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*THE FUNDAMENTAL PRINCIPLES OF ALGEBRA.**

THIS section of the Association, over which I have the honor of being called upon to preside, may be said to be a double section, for it comprises both mathematics and astronomy; as a consequence, the addresses which have been delivered by my predecessors fall into two distinct groups, the mathematical and the astronomical. Of the former class I have had the pleasure of listening to three: Professor Gibbs on Multiple Algebra, Professor Hyde on the Development of Algebra, and Professor Beman on a Chapter in the History of Mathematics. Each of these addresses was devoted to one feature or other of the development of Algebra, and the subject which I have chosen for to-day is another aspect of the same wonderful phenomenon. It is a subject which interests alike the mathematician and the philosopher, and indeed all thinking men, for it concerns the foundations of that science which is generally acknowledged to be the most perfect creation of the human intellect.

I propose then to review historically and critically the several advances which have been made respecting the fundamental principles of algebra. Here I am mindful of the advice which Horace gives a young

* Address by the Vice-President and Chairman of Section A., Astronomy and Mathematics, of the American Association for the Advancement of Science, Columbus meeting, August, 1899.

poet, not to begin his epic at the origin of things, but to hasten on to the event proper; consequently, I shall not go back to the Egyptians, Greeks, Hindoos, or Arabs, but at once proceed to the advances made in the present century.

One of the first results of the differential notation of Leibnitz was the recognition of the analogy between $\frac{d}{dx}$ the symbol of differentiation and the ordinary symbol of algebra; later the same analogy was perceived to hold for Δ_x the symbol of the calculus of finite differences. Guided by this analogy, Lagrange and other mathematicians of the French school, which flourished at the beginning of the century, inferred that theorems proved to be true for combinations of ordinary symbols of quantity might be applied to the differential calculus and the calculus of finite differences. In this way many theorems were enunciated, which appeared to be true, but of which it was thought to be almost impossible to obtain direct demonstration. Gradually, however, the view was reached that the logical connection amounted to more than analogy, and that the common theorems were true because the symbols in the three cases were subject to the same fundamental laws of combination. This advance was principally made by Servais, who enunciated the laws of commutation and distribution.

About the year 1812 a school of mathematicians arose at Cambridge which aimed at introducing the d-ism of the Continent in place of the dot-age of the University; in other words they believed in the practical superiority of the differential notation of Leibnitz over the fluxional notation of Newton. Their attention was naturally drawn to the questions which had sprung from the differential notation; and of the three founders of the school—Babbage, Herschel, Peacock—the last named took up the problem of placing the teaching of algebra more in consonance with the views which had been reached of the nature of symbols. Peacock considered algebra, as then taught, to be more of an art than a science; a collection of rules rather than a system of logically connected principles; and with the object of placing it on a more scientific basis, he made a distinction between arithmetical algebra and symbolical algebra. He treated these names as denoting distinct sciences, and he wrote an algebra in two volumes, of which one treats of arithmetical algebra and the other of symbolical algebra. He thus describes what he means by the former term: "In arithmetical algebra we consider symbols as representing numbers and the operations to which they are submitted as included in the same definitions as in common arithmetic; the signs $+$ and $-$ denote the operations of addition and subtraction in their ordinary meaning only, and those operations are considered as impossible in all cases where the symbols subjected to them possess values which would render them so in case they were replaced by digital numbers; thus in expressions such as $a+b$ we must suppose a and b to be quantities of the same kind; in others like $a-b$, we must suppose a greater than b and therefore homogeneous with it; in products and quotients, like $a\,b$ and $\frac{a}{b}$ we must suppose the multiplier and divisor to be abstract numbers; all results whatsoever, including negative quantities, which are not strictly deducible as legitimate conclusions from the definitions of the several operations must be rejected as impossible, or as foreign to the science."

Here it may be observed that Peacock is not true to his own principle; for $\frac{a}{b}$ is as impossible when b is not a divisor of a, as is $a-b$, when b is not less than a; in neither

case do we get a digital number. He draws the line so as to exclude the fraction as a multiplier but not as a multiplicand; according to his own principle it should be wholly excluded from arithmetical algebra. But arithmetic so restricted would be a very narrow science, and the logical result would be to divide arithmetic itself into an arithmetical arithmetic and a symbolical arithmetic.

He then describes what he means by 'symbolical algebra.' "Symbolical algebra adopts the rules of arithmetical algebra but removes altogether their restrictions; thus symbolical subtraction differs from the same operation in arithmetical algebra in being possible for all relations of value of the symbols or expressions employed. All the results of arithmetical algebra which are deduced by the application of its rules, and which are general in form, though particular in value, are results likewise of symbolical algebra, where they are general in value as well as in form; thus the product of a^m and a^n which is a^{m+n} when m and n are whole numbers, and therefore, general in form, though particular in value, will be their product likewise when m and n are general in value as well as in form; the series for $(a+b)^n$ determined by the principles of arithmetical algebra when n is any whole number, if it be exhibited in a general form, without reference to a final term, may be shown upon the same principle to be the equivalent series for $(a+b)^n$ when n is general both in form and value."

The principle here brought forward was named by Peacock the 'principle of the permanence of equivalent forms'; by means of it the transition is made from arithmetical algebra to symbolical, and at page 59 of 'Symbolical Algebra' it is thus enunciated: "Whatever algebraical forms are equivalent, when the symbols are general in form but specific in value, will be equivalent likewise when the symbols are general in value as well as in form."

One asks naturally, 'What are the limits set to the generality of the symbol?' Peacock's answer is, 'Whatsoever.' In the theory of reasoning the great question is not, 'How do we pass from generals to particulars?' but 'How do we pass from particulars to generals?' The application of general principles is plain enough—the difficulty is in explaining how we arrive at the truth of the general principles. The logician, seeking for light on this question, is apt to turn to exact science, and especially to algebra, the most perfect branch of exact science. Should he turn to Peacock, he finds that all that is offered him is this 'principle of the permanence of equivalent forms'; which, paraphrased, amounts to the following: We find certain theorems to be true when the symbol denotes integer number; let these theorems be true without restriction, and let us try to find the different interpretations which may be put on the symbol. Is not the following attitude more logical? We find certain theorems to be true, when the symbol denotes number; how far and no further may the conception of number be generalized, yet these theorems remain true without any alteration of form?; and, should the conception of number be still further generalized, what is the modified form which the theorems then assume? This is the logical process of generalization, whereas Peacock's process is "essentially arbitrary, though restricted with a specific view to its operations and their results admitting of such interpretations, as may make its applications most generally useful." (Report on Recent Progress in Analysis, p. 194.)

The two processes may be illustrated by their application to the binomial theorem, proved to be true for a positive integer index. According to Peacock's process,

$$(a+b)^n = a^n + na^{n-1}b + \frac{n(n-1)}{1.2}a^{n-2}b^2 +$$

is to be made a theorem in symbolical algebra, whether the series be finite or infinite, and all that remains is to find the different ways in which it may be interpreted. The process of generalization proceeds by steps. For instance, it asks: Will the series retain the same form when n is generalized so as to include any rational fraction? This is one of the questions which Newton proposed to himself, and settled in the affirmative; and it is recorded that he verified the truth of his conclusion by squaring the series for $(1-x^2)^{\frac{1}{2}}$. Peacock's principle does not distinguish divergent from convergent series; it is nothing but hypothesis, and any result suggested by it must stand the test of independent investigation.

An important advance in the philosophy of the fundamental principles of algebra was made by D. F. Gregory, a younger member of the Cambridge school of mathematicians. Descended from a Scottish family, already famous in the annals of science, he early gave promise of adding additional luster to the name; this he accomplished in a brief life of thirty-one years. In 1838 he read a paper before the Royal Society of Edinburgh 'On the Real Nature of Symbolical Algebra,' in which he says: "The light in which I would consider symbolical algebra is, that it is the science which treats of the combination of operations defined not by their nature, that is, by what they are or what they do, but by the laws of combination to which they are subject. And as many different kinds of operations may be included in a class defined in the manner I have mentioned, whatever can be proved of the class generally, is necessarily true of all the operations included under it. This, it may be remarked, does not arise from any analogy existing in the nature of the operations which may be totally dissimilar, but merely from the fact that they are all subject to the same laws of combination. It is true that these laws have been in many cases suggested (as Mr. Peacock has aptly termed it) by the laws of the known operations of number; but the step which is taken from arithmetical to symbolical algebra is, that leaving out of view the nature of the operations which the symbols we use represent, we suppose the existence of classes of unknown operations subject to the same laws. We are thus able to prove certain relations between the different classes of operations, which, when expressed between the symbols are called algebraical theorems. And if we can show that any operations in any science are subject to the same laws of combination as these classes, the theorems are true of these as included in the general case; provided always that the resulting combinations are all possible in the particular operation under consideration."

It will be observed that he places algebra on a formal basis; for its symbols are defined, not to represent real operations, but by laws of combination arbitrarily chosen. In a subsequent paper, however, entitled 'On a Difficulty in the Theory of Algebra,' he practically gave up the formal view, and appears inclined to adopt the realist view instead. He says: "In previous papers on the theory of algebra I have maintained the doctrine that a symbol is defined algebraically when its laws of combination are given; and that a symbol represents a given operation when the laws of combination of the latter are the same as those of the former. This, or a similar theory of the nature of algebra seems to be generally entertained by those who have turned their attention to the subject; but without in any degree leaning on it, we may say that symbols are actually subject to certain laws of combination, though we do not suppose them to be so defined; and that a symbol representing any operation must be subject to the same laws of combination as the

operation it represents." This is a departure from conventional definitions to rules founded upon the universal properties of that which is represented.

In the paper first quoted, Gregory considers five classes of operations. He supposes + and — to be defined by the rules of signs; and he finds in arithmetic a pair of operations which come under it, namely, addition and subtraction; and in geometry another pair, namely, turning through a circumference, and a semi-circumference respectively. But it is instructive to note that the difficulty referred to in the title of the later paper is none other than the view that + and — represent the operations of addition and subtraction; and he there shows that addition (including subtraction) is subject to a couple of very different laws, the commutative and the associative, though he does not use the latter term. It may be observed that the rule of signs applies to × and ÷ also; hence if + and — embraced addition and subtraction, so would × and ÷. The truth of the matter is that in ascending from arithmetic to algebra, we replace the coördinate ideas of *addition* and *subtraction* by the more general idea of *sum* and the subordinate functional idea of *opposite*. Similarly the coördinate ideas of multiplication and division are replaced by the more general idea of a *product* and the subordinate functional idea of *reciprocal*. The symbols — and ÷ then denote opposite and reciprocal respectively, while the ideas of sum and product are not expressed by symbols, but are sufficiently indicated by the manner of writing of the several elements. This difficulty appears to have upset his belief in the existence of classes of operations subject to the same laws of combination, yet totally dissimilar in nature, and without any real analogy binding them together.

According to Gregory, the second class of operations are index operations, subject to the two laws:

$$f_m(a)f_n(a) = f_{m+n}(a) \quad \text{and} \quad f_mf_n(a) = f_{mn}(a).$$

The third class comprises the ordinary symbol of algebra, and the symbols d and Δ of the calculus; they are subject to the distributive law

$$f(a) + f(b) = f(a + b),$$

and to the commutative law

$$f_1f(a) = ff_1(a).$$

The fourth class comprises the logarithmic operations subject to the law

$$f(a) + f(b) = f(ab).$$

The fifth class are the sine and cosine functions, subject to the laws expressed by the fundamental theorem of plane trigonometry, namely, the connection between the sine and cosine of the sum of two angles and the sines and cosines of the component angles.

Following as far as may be the chronological order, we come next to Augustus De Morgan, distinguished for his contributions alike to logic and to mathematics. In his 'Formal Logic' he takes a formal view of the nature of reasoning in general, and in his 'Trigonometry and Double Algebra' he lays down an excessively formal foundation for algebra. Indeed, it may be said that he carries formalism to its logical issue; and, thereby, he renders a service, for its inadequacy then becomes the better evident. In the chapter of the book mentioned, which is headed, 'On Symbolic Algebra,' he thus expresses the view he had arrived at: " In abandoning the meanings of symbols, we also abandon those of the words which describe them. Thus addition is to be, for the present, a sound void of sense. It is a mode of combination represented by +; when + receives its meaning, so also will the word addition. It is most important that the student should bear in mind that, *with one exception*, no word nor sign of arithmetic or algebra has one atom of meaning throughout this chapter, the object of which is symbols, and their laws of

combination, giving a symbolic algebra which may hereafter become the grammar of a hundred distinct significant algebras. If any one were to assert that + and − might mean reward and punishment and *A*, *B*, *C*, etc., might stand for virtues and vices, the reader might believe him, or contradict him, as he pleases, but not out of this chapter. The one exception above noted, which has some share of meaning, is the sign = placed between two symbols, as in $A = B$. It indicates that the two symbols have the same resulting meaning, by whatever steps attained. That A and B, if quantities, are the same amount of quantity, that if operations, they are of the same effect, etc." Let us apply to the theory quoted the logical maxim that the exception proves the rule, *prove* being used in the old sense of test. Well then, I say, because one symbol at least is found to be refractory to the theory, it follows that the theory is fallacious.

De Morgan proceeds to give an inventory of the fundamental symbols and laws of algebra, that for the symbols being 0, 1, +, −, ×, ÷, $(\ )^{(\ )}$ and letters. With respect to it the following questions may be asked: Why should $(\ )^{(\ )}$ be included, while the inverse idea, denoted by *log* is left out? What of the functional symbols *sin* and *cos*? Can they be derived from the above? As − denotes opposite and ÷ reciprocal, what are the signs for sum and product? Can they be derived from the above?

His inventory of the fundamental laws is expressed under fourteen heads, but some of them are merely definitions. The laws proper may be reduced to the following, which he admits are not all independent of one another:

I. Law of signs: $++=+$, $+-$ or $-+=-$, $--=+$,
$\times\times=\times$, $\times\div$ or $\div\times=\div$, $\div\div=\times$.

II. Commutative law: $a+b=b+a$, $ab=ba$.

III. Distributive law: $a(b+c)=ab+ac$.

IV. Index laws: $a^b\times a^c=a^{b+c}$, $(a^b)^c=a^{bc}$, $(ab)^c=a^cb^c$.

V. $a-a=0$, $a\div a=1$.

These last may be called the rules of reduction. What Gregory gave was a classification of the more important operations occurring in algebra; De Morgan professes to give a complete inventory of the laws which the symbols of algebra must obey, for he says "Any system of symbols which obeys these rules and no others, except they be formed by combination of these rules, and which uses the preceding symbols and no others, except they be new symbols invented in abbreviation of combinations of these symbols, is symbolic algebra."

Compare this inventory with Gregory's classification. De Morgan brings × and ÷ under the same rule as + and −; he applies the commutative law to a sum as well as to a product; he introduces the third index law, which makes the index distributive over the factors of the base; he leaves out the logarithmic and trigonometrical principles and introduces what may be called the rules of reduction. From his point of view, none of them are rules; they are laws, that is, arbitrarily chosen relations to which the algebraic symbols must be subject. He does not mention the law pointed out by Gregory, afterwards called the law of association. It is an unfortunate thing for the formalist that a^b is not equal to b^a, for then his commutative law would have full scope; as it is, the index operations prove exceedingly refractory, so that in some of the beautifully formal systems they are left out of account altogether. Here already we have sufficient indication that to give an inventory of the laws which the symbols of algebra *must* obey, is as ambiguous a task as to give an inventory of the *a priori* furniture of the mind.

Like De Morgan, George Boole was a mathematician who investigated and wrote

in the field of logic. The character of the work done by the two men is very different. De Morgan's work bristles with new symbols; Boole uses only the familiar symbols of analysis. The former polished many small stones; the latter raised an edifice of grand proportions. The work done by Boole in applying mathematical analysis to logic necessarily led him to consider the general question of how reasoning is accomplished by means of symbols. The view which he adopted on this point is stated on page 68 of the 'Laws of Thought.'

"The conditions of valid reasoning by the aid of symbols are: *First*, that a fixed interpretation be assigned to the symbols employed in the expression of the data, and that the laws of the combination of these symbols be correctly determined from that interpretation; *Second*, that the formal processes of solution or demonstration be conducted throughout in obedience to all the laws determined as above, without regard to the question of the interpretability of the particular results obtained; *Third*, that the final result be interpretable in form, and that it be actually interpreted in accordance with that system of interpretation which has been employed in the expression of the data."

As regards these conditions it may be observed that they incline toward the realist view of analysis. True, he speaks of interpretation instead of meaning, but it is a fixed interpretation; and the rules for the processes of solution are not to be chosen arbitrarily, but are to be found out from the particular system of interpretation of the symbols. The thoroughgoing realist view is that a symbol stands for some definite notion in the subject analyzed, and that the rules of the analysis are founded upon universal properties of the subject analyzed. The realist view of mathematical science has commended itself to me ever since I made an exact analysis of relationship and devised a calculus which provides a notation for any relationship; can express in the form of an equation the relationship existing between any two persons, and provides rules by means of which a single equation may be transformed, or a number of equations combined so as to yield any relationship involved in their being true simultaneously. The notation is made to fit the subject, and the rules for manipulation are derived from universal physiological laws and the more arbitrary laws of marriage. The basis is real; yet the analysis has all the characteristics of a calculus, and throws light by comparison on several points in ordinary algebra. Its fundamental symbol expresses a relation; and what is the ultimate meaning of the algebraical symbol or of the symbol of the calculus but an operation or relation?

It is Boole's second condition which principally calls for study and examination; respecting it he observes as follows: "The principle in question may be considered as resting upon a general law of the mind, the knowledge of which is not given to us *a priori, i. e.*, antecedently to experience, but is derived, like the knowledge of the other laws of the mind, from the clear manifestation of the general principle in the particular instance. A single example of reasoning, in which symbols are employed in obedience to laws founded upon their interpretation, but without any sustained reference to that interpretation, the chain of demonstration conducting us through intermediate steps which are not interpretable to a final result which is interpretable, seems not only to establish the validity of the particular application, but to make known to us the general law manifested therein. No accumulation of instances can properly add weight to such evidence. It may furnish us with clearer conceptions of that common element of truth upon which the application of the principle depends, and so prepare the

way for its reception. It may, where the immediate force of the evidence is not felt, serve as verification, *a posteriori*, of the practical validity of the principle in question. But this does not affect the position affirmed, viz., that the general principle must be seen in the particular instance—seen to be general in application as well as true in the special example. The employment of the uninterpretable symbol $\sqrt{-1}$ in the intermediate processes of trigonometry furnishes an illustration of what has been said. I apprehend that there is no mode of explaining that application which does not covertly assume the very principle in question. But that principle, though not, as I conceive, warranted by formal reasoning based upon other grounds, seems to deserve a place among those axiomatic truths which constitute in some sense the foundation of general knowledge, and which may properly be regarded as expressions of the mind's own laws and constitution" (p. 68).

We are all familiar with the fact that algebraic reasoning may be conducted through intermediate equations without requiring a sustained reference to the meaning of these equations; but it is paradoxical to say that these equations can, in any case, have no meaning, no sense, no interpretation. It may not be necessary to consider their meaning; it may even be difficult to find their meaning, but that they have a meaning is a dictate of common sense. It is entirely paradoxical to say that, as a general process we can start from equations having a meaning and arrive at equations having a meaning by passing through equations which have no meaning. The particular instance in which Boole sees the truth of the paradoxical principle is the successful employment of the uninterpretable symbol $\sqrt{-1}$ in the intermediate processes of trigonometry. As soon, then, as the $\sqrt{-1}$ occurring in these processes is demonstrated, the evidence for the principle fails. As a matter of fact, the doctrine of algebraists about $\sqrt{-1}$ has long been a dark corner in exact science; and as a consequence it has been made the foundation for all sorts of crank theories. Recently I noticed that an ingenious individual had applied the $\sqrt{-1}$ and its successive powers to construct a mathematical theory of sensation. Before the introduction by Descartes of the geometrical idea of the opposite the use of $-$ in algebra might have been made the foundation for a similar transcendental theory of reasoning. Algebra, as the analysis of quantity in space, has a clear meaning for $\sqrt{-1}$ as the operation of turning through a right angle round a definite or an indefinite axis; in the former case it is vector in nature, because the axis must be specified; in the latter it is scalar in nature, because the axis may be any suitable one. It follows that $-$ denotes turning through two right angles, and this includes 'opposite' as a particular case. Thus an instance is still wanting on which to build the transcendental theory of reasoning enunciated by Boole.

The object of Boole's work, 'The Laws of Thought," is to investigate the fundamental laws of thought, to give expression to them in the symbolical language of a calculus, and upon that foundation to establish the science of logic. In the concluding chapter he considers the light which the inquiry throws on the nature and constitution of the human mind. Now, as a matter of fact, the subject analyzed is quality, and its connection with the nature and constitution of the human mind is nowise more inanimate than is the connection of algebra the science of quantity.

It is interesting to compare Boole's inventory of the symbols and laws for a calculus of reasoning (analysis of quality) with the inventory made by De Morgan for the symbols and laws of algebra (the analysis of quantity). The symbols are the same, ex-

cepting that $(\ )^{(\ )}$ is omitted. The law of signs for $+$ and $-$ is the same, but none is given for $\times$ and $\div$ on account of the ambiguity of the reciprocal; the commutative law applies to both sum and product; the distributive law applies to the product of sums; there are no index laws, excepting the peculiar one $a^2 = a$. The law of reduction $a - a = 0$ remains, but the complementary law $\frac{a}{a} = 1$ is not true in general.

How is the truth or suitability of these laws established? He says that it would be mere hypothesis to borrow the notation of the analysis of quantity, and to assume that in its new application the laws by which its use is governed would remain unchanged; to establish them he investigates the operations of the mind in reasoning as expressed by language, and applies Kant's theory of seeing the general truth in a particular instance. As regards the commutative law it may be remarked that Boole overlooks the fact that two notions may in their definition be coördinate with one another, or subordinate the one to the other, just as in the theory of probability there is a difference between two events which are independent of one another, and two events which are dependent the one on the other; and in the latter case it is not true that the order of the notions is indifferent. This is not the place to enter into a discussion of these so-called laws of thought; I wish merely to point out that Boole's view is essentially that of the realist; the fundamental rules of an analysis are not to be assumed arbitrarily, but must be found out by investigation of the subject analyzed.

Contemporaneously with Boole, and living on the same Emerald Isle, another mathematician spent many days reflecting on the fundamental principles of algebra—Sir W. R. Hamilton. His investigations started from the reading of some passages in Kant's 'Critique of the Pure Reason' which appeared to justify the expectation that it should be possible to construct *a priori* a science of time as well as a science of space. The principal passage is as follows: "Time and space are two sources of knowledge from which various *a priori* synthetical cognitions can be derived. Of this pure mathematics gives a splendid example in the case of our cognitions of space and its various relations. As they are both pure forms of sensuous intuition, they render synthetical propositions *a priori* possible." Thus, according to Kant, space and time are forms of the intellect; and Hamilton reasoned that, as geometry is the science of the former, so algebra must be the science of the latter. He amplifies that view as follows: "It early appeared to me that these ends might de attained by consenting to regard algebra as being no mere art, nor language, nor primarily a science of quantity, but rather as the science of order in progression. It was, however, a part of this conception that the progression here spoken of was understood to be continuous and unidimensional, extending indefinitely forward and backward, but not in any lateral direction. And although the successive states of such a progression might, no doubt, be represented by points upon a line, yet I thought that their simple successiveness was better conceived by comparing them with moments of time, divested, however, of all reference to cause and effect; so that the 'time' here considered might be said to be abstract, ideal or pure, like that 'space' which is the object of geometry. In this manner I was led to regard algebra as the science of pure time, and an essay containing my views respecting it as such was published in 1835." (Preface to 'Lectures on Quaternions,' p. 2.) If algebra is based on any unidimensional subject a difficulty arises in explaining the roots of a quadratic equation when they are imaginary. To get over the difficulty

Hamilton invented a theory of algebraic couplets, but the success of the invention is doubtful. In his presidential address before the British Association the late Professor Cayley said that he could not appreciate the manner in which Hamilton connected algebra with the notion of time, and still less could he appreciate the manner in which he connected his algebraical couplet with the notion of time. Whether Hamilton has effected the explanation or not, it appears to be logically possible, for a complex quantity can be represented by two segments of one and the same straight line.

But, be that as it may, Hamilton was led from algebraic couplets to algebraic triplets and to the problem of adapting triplets to the representation of lines in space. His guiding idea was to extend to space the mode of multiplication of lines in a plane already discovered by Argand, Warren and others; and it was here that he stepped from the time basis to the space basis—that is, passed from a unidimensional to a tridimensional subject, the latter including the former as a special case. To his surprise, he found that the multiplication of two lines in space, either one being expressed in terms of three elements, led to a product composed not of three, but of four elements; and this result he deemed so novel and characteristic that he selected it to give a name to the new method—'Quaternions.' As finally developed, the method rests on a geometrical basis; nevertheless it is the logical generalization of ordinary algebra, for the distinctive theorems of algebra, such as the exponential, binomial and multinomial theorems, have their generalized counterparts in quaternions. Since the time of Gauss, mathematicians have considered double or plane algebra to be the logical generalization of ordinary algebra; now quaternions bears to plane algebra the same logical relation which plane algebra bears to ordinary algebra. It is all algebra in the sense of being the analysis of quantity and the relations of quantities. Any one who admits De Moivre's theorem into algebra is logically bound to admit quaternions as the highest form of algebra. It is a common belief that quaternions has only a remote connection with algebra; that it is only one of several systems of non-commutative algebra, and that the mathematician can get on very well without it. But if the above is the true logical relation, then it must be the duty of every analyst to master its principles. It may be remarked here that the logical relation of quaternions to plane algebra is obscured by the prevalent but erroneous idea that the complex quantities of the form $x + iy$ represent vectors. They really represent, in their planar meaning, coaxial quaternions; that is, x is a scalar and the axis of y is the common perpendicular to the plane. Let, as usual, $w + ix + jy + kz$ denote a quaternion; the complex quantity is identical not with $w + ix$ or $ix + jy$, but with $w + kz$. The fallacy in question almost baffled Hamilton in his attempts at generalization, as may be seen from the account which he gives of the discovery in the *Philosophical Magazine* for 1844.

We shall obtain additional insight into the nature of the fundamental laws of algebra by considering the part which they played in the discovery of the quaternion generalization. In the endeavor to adapt the general conception of a triplet to the multiplication of lines in space Hamilton started out with the principles of commutation, distribution and reduction; but in order that the theorem about the moduli might remain true he soon felt obliged, not indeed to abandon the principle of commutation entirely, but to modify it so as to preserve the order of the factors while leaving the order of combination of the factors commutable. This principle, which had

previously been pointed out by Gregory as an independent principle, he called the law of association. As the principle of commutation was still assumed to apply to the terms of a sum, it followed that the principle of association also applied to them. Here, then, we have an important difference in the inventory of the laws of algebra. According to De Morgan algebra follows all the laws which he enumerated, and them only; but Hamilton showed that the legitimate extension of algebra to space requires the commutative law to be modified in the case of a product. And still further light is obtained on the nature of these laws by considering the way by which Hamilton satisfied himself of the truth of the principle of association. He sought for and obtained a geometrical proof, independent of the principle of distribution, and depending on theorems taken from spherical trigonometry or spherical conics. Thus a notable generalization of algebra was made, not by arbitrary choice of fundamental rules, nor by arbitrary extension of the rules for integer number, but by finding out the universal properties of the subject analyzed.

We have already found that the index operations form a valuable test of the soundness of any theory of algebra. If the method of quaternions is the true extension of algebra to space we expect it to throw new light on these operations. As a matter of fact, most of the works on quaternions ignore the subject or present instead the treatment for the plane. In Hamilton's 'Elements of Quaternions' there is a chapter headed 'On Powers and Logarithms of Diplanar Quaternions,' but what it contains is practically limited to the plane. Why? Because the author believed, and there states, that the fundamental exponential law is not true for diplanar quaternions; that is, for space

$$e^p \times e^q \text{ not } = e^{p+q}.$$

The source of error lies in regarding the sum of indices as commutative, for that amounts to holding that $e^p \times e^q = e^q \times e^p$, which is contrary to the principles of quaternions. Were $p+q$ a sum without any real order of the terms, then we might have an order of factors, that is, we might have

$$(p+q)(p+q) = p^2 + pq + qp + q^2 = p^2 + q^2 + 2Spq.$$

But when the sum has a real order of p, prior to q, then we cannot at the same time, hold that one factor $p+q$ can be prior to another factor $p+q$; for in the expansion we should have the contradiction of p being prior to q and q at the same time prior to p. Hence when p is prior to q the second power is not formed in accordance with the distributive principle; it is $p^2 + 2pq + q^2$. When this is admitted the exponential principle stands, but the commutative principle for a sum of such indices goes, as does also the distributive manner of forming the power of such a sum.

As regards the third index law it is evident from the non-commutability of the factors in general that in space it ceases to be true. The rule of reduction for a sum of terms requires to be modified when the terms have a real order; for $p+q-q=p$, but $q+p-q$ is not equal to p. The term and its opposite must follow one another immediately in order that the reduction may be legitimate. Similarly, in the case of a product the factor and its reciprocal must follow one another immediately in order that the reduction may be legitimate. From these principles the generalization for space of all the fundamental theorems of algebra follows without difficulty, and the theory of logarithms and exponents becomes the most fruitful part of quaternion analysis.

We may now consider briefly how the advance made by Hamilton struck a cotemporary mathematician—Professor Kel-

land, of the University of Edinburgh. It was his custom to teach the elements of quaternions to the students of his senior class, and I remember how all went well till he came to multiplication, where the part played by a vector as a multiplier was likened, in some mysterious manner, to the action of a corkscrew. In the introductory chapter of the 'Introduction to Quaternions' he remaks as follows on the process by which algebra is generalized : " It is only by standing loose for a time to logical accuracy that extensions in the abstract sciences—extensions at any rate which stretch from one science to another—are effected." And further on : " We trust, then, it begins to be seen that sciences are extended by the removal of barriers, of limitations, of conditions on which sometimes their very existence appears to depend. Fractional arithmetic was an impossibility so long as multiplication was regarded as abbreviated addition; the moment an extended idea was entertained, ever so illogically, that moment fractional arithmetic started into existence. Algebra, except as mere symbolized arithmetic, was an impossibility so long as the thought of subtraction was chained to the requirement of something adequate to subtract from. The moment Diophantus gave it a separate existence—boldly and logically as it happened—by exhibiting the law of *minus* in the forefront as the primary definition of his science, that moment algebra in its highest form became a possibility, and indeed the foundation stone was no sooner laid than a goodly building arose on it."

It seems to me that no greater paradox could be enunciated than to say that higher principles in exact science are reached by standing loose for a time to logical accuracy. How long a time does that which is illogical take to become logical? The true process is generalization, not illogical extension. No doubt, the generalized principle may at first be merely an hypothesis, and in that form it may be applied so that it may be verified by its results; but this is not standing loose to logical accuracy.

The same author gives the following account of how Hamilton *extended* algebra to space : " He had done a considerable amount of good work, obstructed as he was, when, about the year 1843, he perceived clearly the obstruction to his progress in the shape of an old law which, prior to that time, had appeared like a law of common sense. The law in question is known as the commutative law of multiplication. Presented in its simplest form it is nothing more than this: 'five times three is the same as three times five'; more generally it appears under the form of $ab = ba$ whatever a and b may represent. When it came distinctly to the mind of Hamilton that this law is not a necessity with the extended signification of multiplication he saw his way clear and gave up the law. The barrier being removed, he entered on the new science as a warrior enters a besieged city through a practicable breech." This account is, of course, inadequate, for Grassmann jumped over the same barrier in the shape of an 'old law,' yet he was unable to deal with angles in space. There is no occasion to speak disrespectfully of the law of commutation; it has its own place; Hamilton did not cast it aside as an obstruction; he modified it for a product of factors having a real order, and the modified form amounts to the law of association.

We shall now go back to another independent source of the development of the principles of algebra—Hermann Grassmann. Like his contemporary, Hamilton, he was remarkable alike for attainments in mathematics and philosophy, and, besides, he made important contributions to philology. No doubt specialists are necessary, but the investigation of the fundamental principles of a science requires one who is

more than a specialist, one who has not only studied a portion minutely, but has also taken a comprehensive glance over the whole. From the preface to the *Ausdehnungslehre* of 1844 we get an insight into the origin and development of his course of investigation, and we find that it was in a manner the reverse of Hamilton's. The former started from a variety of geometrical facts and developed a method which is independent of space, and has perhaps suffered from its *philosophische Allgemeinheit;* the latter started from general philosophical ideas and developed an algebra which is uniquely adapted to space of three dimensions. But, as their subjects were largely the same, their results, so far as they involve truth, must also be capable of unification to a large extent.

In the preface quoted, Grassmann informs us that he started from the treatment of negatives in geometry; he observed that the straight lines AB and BA were opposite, and that AB + BC = AC, whether the point C is beyond B or between A and B. This led him to the principle of geometrical addition—namely, that AB + BC = AC, whether A, B, C are in one straight line or not. It may be remarked here that this principle is all right so long as the components have no real order, such as forces applied at a point or the coördinates of a point; but that it does not apply where the components have a real order, as, for example, the sides of a polygon. In successive addition the straight line from the origin to the end of the polygon is the scalar result, but the area enclosed is another result, which depends on the form of the path.

Then turning to the product in geometry, he adopted the view that the parallelogram is the product of its two sides, whether these are at right angles or not. He next found that the geometrical ideas of a sum and a product which he had adopted satisfied the principle of distribution, but not the principle of commutation so far as the factors of a product were concerned. In the case of the products commutation could be made, provided the sign of the product were changed also—that is, they were subject to negative commutation. Another set of basal facts was taken from the doctrine of the center of gravity. He observed that the center of gravity may be considered as the sum of several points, the line joining two points as the product of the points, the triangle as the product of its three points, and the pyramid as the product of its four points; and from these facts he developed a method similar to the 'Barycentric Calculus,' of Möbius.

He also considered the geometrical meaning of the exponential function. He observed that if a denote a finite straight line and α an angle in a plane through the line, then ae^{α} denotes the line a turned through the angle α. The treatment of angles in one plane is easy, but on attempting to treat of angles in space he encountered difficulties which he was unable to surmount. This fact has been cited as indicating the superiority of Hamilton's method; while that is true, it must not be forgotten that Hamilton failed to generalize the exponential theorem.

What is the view which Grassmann takes of the fundamental principles of algebra? An answer to this question is found in the introduction to the *Ausdehnungslehre* of 1844. He divides the sciences into the real and the formal; the former treat of reality, and their truth consists in the agreement of thought with reality; the latter treat of thought only, and their truth consists in the agreement of the processes of thought with one another. Pure mathematics is the doctrine of forms. As a consequence he is obliged to place geometry under applied mathematics, for it has a real subject, and should anyone think otherwise he must deduce from pure thought the tridimen-

sional character of space. Were space a form of thought, so would be time and motion, and kinematics would also be a part of pure mathematics. So he relegates geometry to the real sciences, and he has a difficulty in retaining arithmetic even, for is it not based on axioms, whereas a formal science is based on conventions?

From the notion of the combination of terms he deduces that the placing of the brackets and the order of the terms may or may not be indifferent. There is a synthetic combination and an analytic combination; when the latter is unambiguous (that is, $a - a = 0$) then the placing of the brackets and the order of the terms is indifferent; synthetic combination is then called addition, and the analytic subtraction. Thus in Grassmann's view the commutative and associative laws are involved in the ideas of addition and subtraction. It may be observed that the old difficulty with subtraction is due to the fact that it is not thoroughly commutative, and that it is only to the generalized idea of composition that the commutative law applies. Besides, to define addition so as to exclude terms having a real order is an arbitrary restriction of algebra.

According to Grassmann's view multiplication is a combination of a higher order; that is, he assumes as the definition of multiplication the distributive principle in the two-fold form

$$(a + b)\,c = ac + bc \text{ and } c\,(a + b) = ca + cb.$$

It may be observed, however, that the true expression for the distributive principle is

$$(a + b)\ (c + d) = ac + ad + bc + bd,$$

which assumes that if there is any real order of the terms there can be only one real order $a\ b\ c\ d$.

As regards the laws of indices he says that involution is a combination of the the third order, and that for the sake of shortness he will omit all consideration of it. Besides, its formal definition would be of no use, for in the nature of things it can be applied only in the special sciences through real definitions. This failure to treat of the index laws tells against his whole theory of the nature of algebra. In fact, these laws are the touchstone whereby the soundness of any theory of the foundations of algebra may be tested.

In 1867 Hermann Hankel published his 'Theory of Complex Numbers.' The full title of the work is '*Theorie der complexen Zahlensysteme insbesondere der gemeinen imarginären Zahlen und der Hamilton'schen Quaternionen nebst ihrer geometrischen Darstellung.*' He had studied the writings of both Hamilton and Grassmann, and the aim of the book is to give a complete theory of the several systems, uniting them all under the notion of complex number. From the title we gather that he considered the algebraic imaginaries and the Hamiltonian quaternions as two distinct systems, formal in their nature, but having a representation in space. He begins with positive integer numbers, and finds from a consideration of the notion that the addition of such numbers satisfies the two laws of association and commutation, which he treats as independent of one another. But as regards the notion of the multiplication of such numbers he says that the truth of the commutative law or of the associative law is not self-evident; that the former law can be proved by a geometric construction in a plane, and the latter by a geometric construction in space. As regards the distributive law he says merely that it is a universal property of multiplication. As regards the base and index relation he says that neither the commutative law nor the associative law applies; he enunciates the same three index laws as De Morgan, but does not say whether they are self-evident or require a proof by geometric construction. Here, then, in a pro-

fessedly scientific work, some of these fundamental laws are treated as self-evident, others as requiring geometric proof, and others yet are merely enunciated. If in the case of multiplication the commutative law requires proof, so does it also in the case of addition, for it is just as self-evident that $2 \times 3 = 3 \times 2$ as that $2 + 3 = 3 + 2$.

The manner in which Hankel passes from arithmetic and arithmetical algebra to general algebra is as follows: Algebra, being formal mathematics, can be founded on any system of independent rules; but, in order that its results may be interpretable and that it may be capable of application, it is found convenient to choose the system of fundamental rules satisfied by common arithmetic; in other words, the laws of integer arithmetic are made the laws of algebra. This he calls the 'principle of the permanence of the formal laws,' and enunciates as follows (p. 11): "If two expressions stated in terms of the general symbols of arithmetical algebra (*arithmetica universalis*) are equal to one another they shall remain equal to one another when the symbols cease to denote simple magnitudes and the operations receive any other meaning." Peacock speaks of the permanence of equivalent forms; Hankel of the permanence of the formal laws. Peacock says, "Let any general equivalence in arithmetical algebra be true also in universal algebra"; Hankel says, "Let the fundamental laws of the former be made the fundamental laws of the latter." Hankel gives a more scientific form to what was meant by Peacock.

However, Hankel labors under a logical difficulty from which Peacock was exempt, for he does not take the laws of arithmetical algebra without exception; he rejects the commutative law for a product, in order that quaternions may be included among his complex numbers. But, it may be asked, why not reject the commutative law for addition also? So far as arithmetical algebra is concerned they stand on the same basis. If, as has been shown, the sum of quaternion indices is not commutative we are logically bound, on his principles, to reject the commutative rule for addition also. We are reduced to the alternative: the choice of the fundamental rules is arbitrary, or else they must be founded on the properties of the subject analyzed. The permanence of the formal laws is nothing but hypothesis, and in the case of any generalization must be tested by real investigation.

One of the clearest thinkers on mathematical subjects in recent times was Professor Clifford, who like several of the mathematical philosophers we have spoken of, was cut down in the midst of his scientific activity. In his posthumous work entitled 'The Common Sense of the Exact Sciences' there are chapters on number and quantity in which he explains his views of the fundamental principles of algebra. He starts out from the principle, which he attributes to Cayley and Sylvester, that the number of any set of things is the same in whatever order we count them, and deduces from it, by means of diagrams, the commutative and associative rules for positive integer number. He says that they amount to the following: "If we can interchange any two consecutive things without altering the result then we may make any change whatever in the order without altering the result." It may be remarked that this shows that the commutative and associative properties are not independent, but that the former involves the latter. He next shows, by a diagram, that the distributive rule is true for the two forms $a(b + c) = ab + ac$ and $(b + c)a = ba + ca$, but he does not consider the complete form of the rule $(a + b)(c + d) = ac + ad + bc + bd$.

As regards the impossible subtraction and division he says (p. 33): "Every operation in mathematics that we can invent,

amounts to asking a question, and this question may or may not have an answer according to circumstances. If we write down the symbols for the answer to the question in any of those cases where there is no answer, and then speak of them as if they meant something, we shall talk nonsense. But this nonsense is not to be thrown away as useless rubbish. We have learned by very long and varied experience that nothing is more valuable than the nonsense which we get in this way; only it is to be recognized as nonsense, and by means of that recognition made into sense. We turn the nonsense into sense by giving a new meaning to the words or symbols which shall enable the question to have an answer that previously had no answer."

This is the true phenomenon in algebra; it is more logical than its framer. How can it be possible, unless the algebraist founds his analysis upon real relations? It is the logic of real relations which may outrun the imperfect definitions and principles of the analyst and make it necessary for him to return to revise them.

To get over the impossible subtraction he introduces instead of the discrete unit supposed by number, the idea of a step, making plus mean 'forwards' and minus 'backward.' The summing of steps is independent of the order in which they are taken, and a minus step is just as independent as a plus step. When these symbols occur in multipliers he gives them, not the meaning of 'forwards' and 'backwards,' but that of 'keep' and 'reverse.' He gives them these meanings in addition to their former meanings, and leaves it to the context to show which is the right meaning in any particular case. It may be remarked that it is doubtful whether in any case two distinct meanings can be given to a symbol at one and the same time without producing confusion. It seems to me, as already stated, that the most general meanings of + and − are the angular ideas of an even and an odd number of semi-circumferences, but this reduces in certain cases to the linear ideas of direct and opposite.

From the idea of step he passes to the idea of operation, on the theory that a product may be composed either of a step and an operation or of two operations. As a matter of fact, an operation is merely a relationship which may subsist between two quantities; and we may have two distinct products, one expressing a related quantity, the other a compound relationship. The analysis of operations is a special part of the more general analysis of relationships. According to Clifford's view, because a sum of operations of the kind considered is independent of the order of the operations, it follows that

$$a + b = b + a \qquad ab = ba$$
$$a(b + c) = ab + ac \qquad (a + b)c = ac + bc.$$

As regards the advance from numbers to quantity he says ('Philosophy of the Pure Sciences,' p. 240): "For reasons too long to give here, I do not believe that the provisional use of unmeaning arithmetical symbols can ever lead to the science of quantity; and I feel sure that the attempt to found it on such abstractions obscures its true physical nature. The science of number is founded on the hypothesis of the distinctness of things; the science of quantity is founded on the totally different hypothesis of continuity. Nevertheless, the relations between the two sciences are very close and extensive. The scale of numbers is used, as we shall see, in forming the mental apparatus of the scale of quantities, and the fundamental conception of equality of ratios is so defined that it can be reasoned about in terms of arithmetic. The operations of addition and subtraction of quantities are closely analogous to the operations of the same name performed on numbers, and follow the same laws. The composi-

tion of ratios includes numerical multiplication as a particular case, and combines in the same way with addition and subtraction. So close and far-reaching is this analogy that the processes and results of the two sciences are expressed in the same language, verbal and symbolical, while no confusion is produced by this ambiguity of meaning, except in the minds of those who try to make familiarity with language do duty for knowledge of things."

What is the analogy here spoken of? It cannot be a mere rhetorical analogy; it is a true logical analogy. But what is a logical analogy, except that the subjects have something in common, which is the basis of the common properties. The logical relation of number to quantity is that of subordination; we cannot pass deductively from the former to the latter, but we can pass deductively from the latter to the former. It is easy to pass downwards from quantity to number; the difficulty is in passing upwards from number to quantity.

The most elaborate treatise on algebra written in the English language within recent times is Chrystal's 'Text-book of Algebra,' published in two volumes. The task which the author sets before himself is the same as that which Peacock undertook —namely, to place the teaching of the elements of algebra on a scientific basis, and abreast of what may be called the technical knowledge of the day. In the first volume he starts out with the idea of building up the science on the three laws of association, commutation and distribution, the two former being applicable to addition and subtraction, multiplication and division, and the third to multiplication. The view which he takes of these laws is expressed by the phrase 'canons of the science,' as is evidenced by the following passage: "As we have now completed the establishment of the fundamental laws of ordinary algebra, it may be well to insist once more upon the exact position which they hold in the science. To speak, as is sometimes done, of the proof of these laws in all their generality is an abuse of terms. They are simply laid down as the canons of the science. The best evidence that this is their real position is the fact that algebras are in use whose fundamental laws differ from those of algebra. In the algebra of quaternions, for example, the law of commutation for multiplication and division does not hold generally."

If it is an abuse of terms to speak of the proof of these laws why does Hamilton devote page upon page to the proof of the associative law for a product of quaternions? He is not content with laying it down as a canon; he investigates whether it corresponds to nature. No doubt, the function of the expositor is different from that of the investigator; the latter must establish principles in the best way he can; the former may proceed deductively from these principles as the axioms of the science. But the idea of 'canon' involves something arbitrary and formal which is not involved in the idea of an 'axiom.'

But if we turn to the second volume we find evidence against the canonical nature of these laws, for the author admits that they must be modified within the bounds of algebra itself. The law of association cannot be applied to the terms of an infinite series, unless it is convergent; the law of commutation cannot be applied to the terms of an infinite series, unless it is absolutely convergent; and the law of distribution requires modification when applied to the product of two infinite series. If, in any case, the so-called canons are modified there must be some higher authority to which appeal is made. The only conclusion left is that the rules in question are not canons at all, excepting in so far as they represent properties of the subject analyzed.

I may here refer to the prevalent doctrine

that the number-system of arithmetic closes with the complex number, and that the operations of algebra give no indication of any higher imaginary form. For instance, in an article on 'Monism in Arithmetic,' Professor Schubert says: "In the numerical combination $a + ib$, which we also call number, we have found the most general numerical form to which the laws of arithmetic can lead, even though we wished to extend the limits of arithmetic still further. * * * With respect to quaternions which many might be disposed to regard as new numbers it will be evident that though quaternions are valuable means of investigation in geometry and mechanics they are not numbers of arithmetic, because the rules of arithmetic are not unconditionally applicable to them." When the plane of the complex quantity is that of the axes of x and y it is true that no higher form appears, because in multiplication we get only k and k^2, which is -1. But when Hamilton took for the common plane a general plane passing through the axis of x he immediately encountered a higher form jk, and the problem resolved itself into finding the meaning of that new imaginary combination. He had a great difficulty in emerging out of 'Flatland,' but he succeeded in doing it. The reason given for excluding the quaternion cannot apply, for it would exclude infinite series, as the rules of arithmetic are not unconditionally applicable to them.

Last year there appeared the first volume of a 'Treatise on Universal Algebra,' by Mr. Whitehead, of Trinity College, Cambridge. By universal algebra the author means the various systems of symbolic reasoning allied to ordinary algebra, the chief examples being Hamilton's Quaternions, Grassmann's Calculus of Extension and Boole's Symbolic Logic. The author does not include ordinary algebra in his treatment, and the main idea of the work is not unification of the methods, nor generalization of algebra so as to include them, but a detailed study of each structure, to be followed by a comparative anatomy. In this idea of comparative anatomy there is involved the assumption that these methods are essentially distinct and independent. But that they overlap to a large extent is very evident.

The author preaches the view of the extreme formalist; nevertheless, at various places he makes admissions which are very damaging to it. As regards the fundamental rules he says: "The justification of the rules of inference in any branch of mathematics is not properly part of mathematics; it is the business of experience or philosophy. The business of mathematics is simply to follow the rules. In this sense all mathematical reasoning is necessary; namely, it has followed the rule." Must the mathematician wait for the experimenter or the philosopher to justify the rules of algebra? Was it no part of Hamilton's business to test whether the associative law is true of a product of spherical quaternions? To advance the principles of analysis is surely the special work of the mathematician; to follow the rules discovered is work of a lower order.

Mr. Whitehead thus describes a calculus: "In order that reasoning may be conducted by means of substitutive signs it is necessary that rules be given for the manipulation of the signs. The rules should be such that the final state of the signs after a series of operations according to rule denotes, when the signs are interpreted in terms of the things for which they are substituted, a proposition true for the things represented by the signs. The art of manipulation of substitutive signs according to fixed rules, and of the deduction therefrom of true propositions, is a calculus." By substitutive sign is meant one such that in thought it takes the place of that for which it is sub-

stituted. He quotes with approval a saying of Stout's that a word is an instrument for thinking about the meaning which it expresses, whereas a substitutive sign is a means of not thinking about the meaning which it symbolizes; and he adds that the use of substitutive signs in reasoning is to economize thought.

It seems to me that a sign economizes thought in precisely the same way that a word economizes thought, but to greater degree. A word is introduced to dispense with a long phrase or description, and in using the word one no more thinks of its meaning than in using an algebraic symbol does one think of the particular meaning it is made to stand for, for the time being. There seems to be a lurking fallacy that thought is economized by dispensing with it altogether. I prefer the saying of Clifford, with reference to $(a+b)^2 = a^2 + 2ab + b^2$ and its expression in English: "Two things may be observed on this comparison—first, how very much the shorthand expression gains in clearness from its brevity; secondly, that it is only shorthand for something which is just straightforward common sense and nothing else. We may always depend upon it that algebra which cannot be translated into good English and sound common sense is bad algebra."

In his statement of the fundamental principles of algebra Whitehead follows Grassmann to a large extent. He divides them into two classes, the general and the special; the former apply to the whole of ordinary and universal algebra; the latter apply to special branches only. The general principles are as follows: Addition follows the commutative and associative laws; multiplication follows the distributive law, but does not necessarily follow the commutative and associative laws. The theory looks beautiful and plausible, but it does not stand the test of comparison with actual analysis, for quaternions is one of the principal branches of universal algebra, and in it the addition of indices is in general non-commutative, and the power of a binomial of indices is not formed after the distributive law.

But in addition to this formal bond we find in the book another bond uniting the several parts into one whole. In the preface Mr. Whitehead says: "The idea of a generalized conception of space has been made prominent in the belief that the properties and operations involved in it can be made to form a uniform method of interpretation of the various algebras. Thus it is hoped in this work to exhibit the algebras, both as systems of symbolism and also as engines for the investigation of the possibilities of thought and reasoning connected with the abstract general idea of space." The chance for any arbitrary system of symbolism applying to anything real is very small, as the author admits; for he says that the entities created by conventional definitions must have properties which bear some affinity to the properties of existing things. Unless the affinity or correspondence is perfect, how can the one apply to the other? How can this perfect correspondence be secured, except by the conventions being real definitions, the equations true propositions and the rules expressions of universal properties? The placing of the algebra of logic on a space basis has been criticised, but in reply it may be pointed out that logicians have been accustomed ever since the time of Euler to prove their principal theorems by means of diagrams.

Our conclusion about the fundamental rules of algebra is: If the elements of a sum or of a product are independent of order, then the written order of the terms is indifferent, and the product of two such sums is the sum of the partial products; but when the elements of a sum or of a product have a real order, then the written

order of the elements must be preserved though the manner of their association may be indifferent, and a power of a binomial is then different from a product. This applies whether the sum or product occurs simply or as the index of a base.

Descartes wedded algebra to geometry; formalism tends to divorce them. The progress of mathematics within the century has been from formalism towards realism; and in the coming century, it may be predicted, symbolism will more and more give place to notation, conventions to principles and loose extensions to rigorous generalizations.

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PROCEEDINGS OF THE BOTANICAL CLUB OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE AT COLUMBUS, AUGUST 21-24, 1899.

THE Botanical Club met in the room assigned for the meetings of Section G pursuant to the resolution adopted by the Boston meeting, Tuesday morning at 9 o'clock, Dr. Byron D. Halsted presiding. The sessions were continued at that hour each morning, and completed by a meeting at 1:30 p. m., Thursday. In the absence of the Secretary, A. D. Selby was chosen Secretary *pro tempore.*

The attendance and interest in the proceedings of the Club were very satisfactory. The number of papers read was quite equal to the time secured for them.

Under the title 'A Greasewood Compass Plant,' Dr. C. E. Bessey reported that on the high, western Nebraska foot-hills a shrubby species of *Sarcobatus* was observed to bear its leaves in an upright position, with their blades parallel to the meridian. Specimens were obtained for microscopic examination.

The same author gave an account of 'A Visit to the Original Station of the Rydberg Cottonwood.' "This species (*Populus acuminata* Ryd.) was discovered a few years ago in Roubedean Township, in Scott's Bluff County, in western Nebraska, in Carter Cañon. This is a broad cañon bounded by high pine-covered buttes, and in the bottom of the broad cañon is a narrower one fringed with deciduous trees—box elder, elm, cottonwood, willow, plum, red cedar, etc., and among them are clumps of the Rydberg cottonwood. The trees are symmetrical and of much greater beauty than those of the common cottonwood. When old the bark of the trunk is light-colored and very deeply fissured.

Dr. N. L. Britton reported to the Club that Mr. and Mrs. A. A. Heller, who were sent to Porto Rico last winter as collectors for the New York Botanical Garden, had returned, having secured about 1,400 numbers, representing probably over 1,200 species, and over 6,000 specimens of plants. They are now being studied.

Dr. William Saunders gave a brief account of 'The Arboretum and Botanic Garden of the Central Experimental Farm at Ottawa, Canada, established in 1889.' During that year 200 species and varieties of woody plants were planted in botanical groups. Additions were made from year to year, and by the end of 1894 the collection included about 600 different species and varieties. Since 1894 progress has been much more rapid, and up to the present time the total number of species under test or which have been tested is 3,071—of these 1,465 have been found hardy, 320 half hardy, 229 tender, 307 winter-killed and 740 have not been tested long enough to admit of an opinion on their hardiness. Where specimens pass the winter uninjured, or with very small injury to the tips of the branches only, they are classed as hardy. When killed back one-fourth to one-half, half-hardy; when killed to the snow-line, tender. A considerable collec-

tion of hardy herbaceous plants has also been brought together, consisting of about 1,100 species and varieties.

'Tomato Fruit-Rot' was discussed by Professor F. S. Earle, Auburn, Alabama. This disease occurs in all parts of the country where the tomato is grown. It has been attributed to a fungus, *Macrosporium Tomato*. Jones and Grout have shown that this fungus is a saprophyte and is not the cause of the disease. In the earliest stages the disease appears as a watery discoloration of the layer just beneath the cuticle. A little later this discolored area becomes covered with minute, sticky drops. These swarm with bacteria. Pure cultures of this germ will promptly produce the disease when introduced under the skin of a sound tomato. The disease cannot be induced by inoculating the flowers nor by smearing it on the surface of the fruit. When the same germ is introduced deeply in the tissues of the fruit, as by the boll-worm, it produces a watery rot.

The germ is non-spore-bearing, motile Bacillus. It is strictly äerobic. It grows readily on the surface of peptone agar and on boiled potato, feebly on raw potato and in milk. It fails to grow on strawberries, apples and cabbage. The disease is probably spread through the agency of some small insect. A species of Thrips has been observed in suspicious connection with it, but its agency in crossing it is not proved.

'On two hitherto confused species of *Lycopodium*,' Professor Francis E. Loyd spoke of *Lycopodium complanatum* and the so-called variety *chamæcyparissus*, having been found by the speaker in southern Vermont. A study of their characters shows them to be quite distinct, specifically.

'Some of the Investigations on Grasses and Forage Plants in Charge of the Division of Agrostotogy, U. S. Department of Agriculture,' were spoken of by Thomas A. Williams. He called attention more particularly to the field and coöperative experimental work now in progress in the West and South, and spoke of the need of varieties of grasses and forage plants better adapted to use on (*a*) Dry, arid soils, (*b*) Saline and alkaline soils, (*c*) High elevations. Promising forms for cultivation belonging to such genera as *Agropyron*, *Poa*, *Festuca*, *Bromus* and *Bouteloua* are being studied; also selected strains of such commonly cultivated sorts as timothy and Kentucky blue-grass. He asked the coöperation of those interested that the work may be furthered and bettered.

'The Wilting of *Cleome integrifolia*' was mentioned by Dr. C. E. Bessey, calling attention to the fact that when the plant is supplied with too little water its leaflets stand erect. 'The Powdery Mildew of *Polygonum aviculare*' was discussed by the same author, noting its earlier appearance this season, 1899, in Nebraska and the dwarfing effect in the host.

Dr. N. L. Britton presented 'Notes on the Northern Species of *Celtis*.' The speaker discussed the differential characters of *Celtis occidentalis* L. and C. *Crassifolia* Lam. as illustrated by trees in the vicinity of Columbus.

Dr. Britton also made 'Remarks on some Species of *Quercus*.'

Attention was called to the finely developed trees of *Quercus acuminata* about Columbus, both in its typical narrow-leaved form and in the numerous obovate-leaved trees; the character of bark and acorns of the two were commented upon.

'Ohio Stations for Lea's Oak' was the subject of a report by Dr. W. A. Kellerman. A fourth station in Ohio for Lea's Oak was discovered recently at Cedar Point, near Sandusky, Erie county. Two trees only were detected, and they were surrounded by many Black Oaks (*Q. velutina*), and still more numerously by the Shingle Oaks (*Q.*

imbricaria). Other species of oak occur on the Point. The two trees were a few rods apart; their trunks were about twelve and twenty inches in diameter. The other stations known are Cincinnati, Brownsville, Licking county (the tree cut down in 1892, but sprouts from the stump are growing), and Columbus.

'Labèls for Living Plants' were likewise discussed by Dr. Kellerman. He exhibited a modified form of the printed label for living plants, already described before the Botanical Club. In these he uses a printed card label in metal holder covered by mica sheets of proper size. This label in suitable sizes is adapted to use in the greenhouse or out-of-doors.

Under the title 'The Introduced Species of *Lactuca* in Ohio,' A. D. Selby spoke of the recent discovery of plants of *Lactuca saligna* L., south of Dayton; this was first collected in 1898. This species has been studied during a second season and is clearly a distinct species from the pinnatifid-leaved forms of *L. Scariola* L. It is characterized by the slender, twiggy growth, the absence of spines and other minute characters. Photographs were shown to the club. This is seemingly the first recorded occurrence of *Lactuca saligna* in the United States. It seems liable to become widely dispersed.

The same author spoke of some peculiarities of the yearly reappearance of *Plasmopara Cubensis* (B. & C.). This happens in Ohio, usually August 12th to 20th, not earlier, though possibly later the present year. The explanation of this phenomenon was asked; it seems peculiar in the absence of known oöspores.

'What shall we regard as Generic Types?' was discussed by Dr. L. M. Underwood. He reviewed, in the light of his work on the ferns, the method of residues as applied to inclusive genera when they are separated. Another method was suggested, that of regarding the first described species under the genera as the type of the genus. The Linnæan or pre-Linnæan genera must in this method be studied from their pre-Linnæan history. Professor Underwood was inclined to support the latter method.

After a discussion of the paper it was unanimously *Resolved:* "That the question of the determination of generic types be referred to the Committee on Nomenclature, with the request that the Committee submit a report thereupon at the next meeting."

'A Brief Embryological Study of *Lactuca Scariola* L.' was presented by J. W. T. Duvel. This paper showed very briefly the development of the individual pollen grains from the chains of pollen mother cells. It likewise treated of the entrance of the pollen tube into the embryo sac, illustrating the ejection of the sperm nucleus and its union with the oöspore; this union usually takes place near the center of the embryo sac, though such union may take place adjacent in the pollen tube.

'The Position of the Fungi in the Plant System' was the subject of a paper communicated by Professor H. L. Bolley. He submitted that the reported growth of bacteria on a purely mineral medium would call in question the degenerate character of the group. But in his own work he had found that on cultures made by washing water-glass in running water and then in distilled water other fungi still flourished.

Professor A. D. Hopkins presented some 'Botanical Notes by an Entomologist.' He remarked upon the discriminating power of insects as between allied species of conifers, etc., and reported the occurrence of a Larix Swamp in West Virginia.

Professor L. C. Corbett exhibited and illustrated the use of 'A Device for Registering Plant Growth.' This had developed from the author's work at Cornell University and in its completed form is a most satisfactory

instrument. It was exhibited at the request of Dr. J. C. Arthur, by whom the instrument is offered.

Professor O. F. Cook gave 'Notes on Some of the Work of the Division of Botany of the U. S. Department of Agriculture.' The investigations of poisonous plants and the resulting publications were noticed, also the work of the Seed Laboratory reporting upon the germinating power and purity (freedom from weeds and other adulterations) of commercial seeds. Variety tests, particularly of garden vegetables, including recent introductions from abroad are also in progress, and the use of a large tract of land on the Potomac Flats has recently been secured for the purpose. The Section of Seed and Plant Introduction is conducting the importations from abroad and has had, during the past year, agricultural explorers in Japan, Russia and the Mediterranean region. The introduction of new cereals, including rice, garden vegetables, dates and the fig insect (*Blastophaga*) are among the most important items secured. In connection with this work a collection of economic plants is a necessity and has been supplied by an herbarium of ecomomic plants.

Professor W. J. Beal told of the Botanical Club organized by the students and teachers of the Michigan Agricultural College. This has an order of procedure similar to the Botanical Club of the A. A. A. S. Dr. Beal also spoke of the introduction and persistence of *Cabomba Caroliniana* on the grounds of the Michigan Agricultural College.

Professor A. S. Hitchcock spoke of the peculiar distribution of several swamp plants in Kansas and illustrated by maps. Their scattered and unexpected occurrence was remarked upon. He also exhibited first-year results of certain wheat crosses.

Two or three other titles were passed, owing to the early departure or absence of the authors. At the close of the session of the Club on Thursday afternoon, N. L. Britton, A. S. Hitchcock and O. F. Cook, comprising the Committee on Nominations, reported, and the Club elected the following officers:

President—Professor F. S. Earle, Auburn, Alabama.

Vice-President—A. D. Selby, Wooster, Ohio.

Secretary—Professor F. E. Lloyd, New York City.

Upon motion, the cordial thanks of the Botanical Club were extended to Professor Kellerman for excellent arrangements; to the Local Committee of the Association for badges, and to the authorities of the Ohio State University for many courtesies enjoyed.

It was remarked by Dr. Britton that the twenty-seven titles of papers presented before the Botanical Club, those before Section G and those presented before the Botanical Society of America show a greater number than in any other science represented at the meeting. This he thought was an augury of widespread botanical activity; it also indicates that but for the affiliated societies Section G would have been overwhelmed.

On motion, the Club adjourned to meet at 9 o'clock Tuesday morning of the next meeting of the American Association for the Advancement of Science.

A. D. SELBY,
Acting Secretary.

THE AMERICAN MICROSCOPICAL SOCIETY.

THE twenty-first annual meeting of the Society was held in Columbus, Ohio, August 17th, 18th and 19th; and, though not largely attended, it was an occasion of good fellowship and enthusiasm.

Among the papers read and discussed were the following: An Expedient in Difficult Resolution, by R. H. Ward, Troy, N. Y. The Relation of Cancer to Defective

Development, by M. A. Veeder, Lyons, N. Y. Notes on Laboratory Technic, by S. H. Gage, Ithaca, N. Y. The Present Status of Scientific Bibliography, by Henry B. Ward, Lincoln, Nebr. The Reaction of Diabetic Blood to some of the Aniline Dyes, by V. A. Latham, Chicago, Ill. Notices of Some Undescribed Infusoria, by J. C. Smith, New Orleans, La. Modern Conceptions of the Structure and Classification of Diatoms, by Charles E. Bessey, Lincoln, Nebr. Comparative Structure of the Soft Palate, by W. F. Mercer, Ithaca, N. Y. A New Microscope Stand, by A. G. Field, Des Moines, Ia. The Eyes of Typhlomage from the Artesian Well at San Marcos, Texas, by C. H. Eigenmann, Bloomington, Ind. Methods Employed in the Study of the Chiasma of *Bufo vulgaris* by B. D. Myers, Ithaca, N. Y. Indexing, Cataloguing and Arranging Microscopical Literature and Slides, by R. H. Ward, Troy, N. Y. Notes on New Genera of Water Mites, by R. H. Wolcott, Lincoln, Nebr. *Notogonia Ehrenbergii*, Perty, by J. C. Smith, New Orleans, La. Limnobiology and its Problems, by Henry B. Ward, Lincoln, Nebr. The Plankton of Echo River, Mammoth Cave, by Charles A. Kofoid, Urbana, Ill.

One afternoon session was devoted to a symposium on the use of the microscope by teachers and private workers, in which the topic of Animal Histology was presented by Professor Gage, that of Bacteriology by Professor Bleile, and that of Botany by Professor Bessey. On Thursday evening the Society listened to the annual address of the President, Dr. William C. Krauss, of Buffalo, N. Y., on the subject: 'Some Medico-legal Aspects of Diseased Cerebral Arteries.'

The Treasurer's report showed that the Society closed the year practically even, and that nearly one hundred dollars had been added to the Spencer-Tolles Fund, making it now $653.36. When the fund reaches $1,000 it is expected to make use of the income for an annual prize or grant for microscopical research.

The following officers were elected for the coming year:

Professor A. M. Bleile, Columbus, Ohio, President.
Professor C. H. Eigenmann, Bloomington, Ind., Vice-President.
Dr. M. A. Veeder, Lyons, N. Y., Vice-President.
J. C. Smith, New Orleans, Treasurer.
Magnus Pflaum, Pittsburg, Pa., Custodian.

As elective members of the Executive Committee:
Dr. W. W. Alleger, Washington, D. C.
Dr. A. D. Kerr, Buffalo, N. Y.
B. D. Myers, Ithaca, N. Y.

The Society was tendered an evening reception by Mr. J. F. Stone, who showed a fine series of views taken on his trip through the Grand Cañon of the Colorado River. The local committee gave visiting members and ladies a trolley ride around the city, besides providing in many other ways for the success of the meeting.

HENRY B. WARD,
Secretary.

PROCEEDINGS OF THE SIXTEENTH ANNUAL CONVENTION OF THE ASSOCIATION OF OFFICIAL AGRICULTURAL CHEMISTS, HELD AT SAN FRANCISCO, JULY 5-7, 1899.

THE sixteeenth meeting of the Association of Official Agricultural Chemists was held in San Francisco, July 5th to 7th inclusive, under the presidency of Dr. R. C. Kedzie, Chemist of the Agricultural Experiment Station of Michigan. There was a large attendance of agricultural chemists, not only from the Pacific, but also from the Atlantic coast. On account of the early date at which the Association met, making only a little over eight months from the time of the last meeting, it was found that many of the referees and their collaborators had not the time or the opportunity in which to complete the work intrusted to them. Anticipating this difficulty, the Secretary, several weeks in advance of the meet-

ing, suggested to the referees that in lieu of their regular reports on the results of the prescribed analytical work, or in addition to these, they give a brief historical *résumé* of the progress of the subject assigned to each of them since the formation of the Association.

The Secretary of the Association adopted the suggestion also in his report, giving a historical report of the Association of Official Agricultural Chemists. In this sketch it was shown that the Association was formed as a result of a meeting called by the Commissioner of Agriculture of the State of Georgia in 1880. This convention met in the library of the Department of Agriculture in Atlanta, July, 1880, and organized by electing the Hon. J. T. Henderson, Commissioner of Agriculture for the State of Georgia, President. After several days spent in valuable discussion, the convention adjourned to meet in Boston, with the Association for the Advancement of Science, on the 27th of August, 1880.

The third meeting of this Association was held, in connection with the Association for the Advancement of Science, in Cincinnati, beginning August 18, 1881.

After the adjournment of the Cincinnati meeting the interest in the collaboration of the agricultural chemists seemed to die out. It was only after three years that the fourth meeting was called by Mr. Henderson. This meeting assembled in Atlanta, May 15, 1884. After three days spent in convention work an adjournment was made to meet again in September in Philadelphia, in connection with the Association for the Advancement of Science. It was at this meeting in Philadelphia that the Association assumed its present organization. The meeting was held September 8, 1884, Dr. E. H. Jenkins being elected Chairman. The name, Association of Official Agricultural Chemists, was adopted at this meeting and likewise the constitution, which has undergone very little change since. The formal organization took place September 9, 1884.

From that time to the present the Association of Official Agricultural Chemists has been under the patronage of the United States Department of Agriculture; its meetings have usually been held in Washington and its Proceedings have been published as bulletins of that Department.

At first the methods of analysis were incorporated with the Proceedings, but later they were collected together under a separate cover, in which form they are now published.

The successive Presidents of the Association have been as follows: S. W. Johnson, H. W. Wiley, E. H. Jenkins, P. E. Chazal, J. A. Myers, M. A. Scovell, G. C. Caldwell, N. T. Lupton, S. M. Babcock, E. B. Voorhees, H. A. Houston, B. F. Ross, Wm. Frear and A. L. Winton.

The President, Dr. Kedzie, gave an interesting address on the subject of foods and food adulterants. This address was read at the joint meeting of the Association of American Agricultural Colleges and Experiment Stations and the Association of Official Agricultural Chemists, immediately following the address of Dr. Armsby, of the first-named Association.

In the regular proceedings of the Association the report of the referee on potash was read by Mr. B. B. Ross, which included an historical account of the methods of analyzing potash since the formation of the Association. Mr. Ross also gave an interesting table showing the results of the comparative analyses of potash samples made during the preceding year.

On the second day a report was made by the referee on soils, Mr. B. L. Hartwell, of Rhode Island, giving the results of comparative studies of the composition of the soils from different parts of the country and by the different methods of determination.

The report of the referee on foods and feeding stuffs, by Thorn Smith, was read by the Secretary, in the absence of the referee and his associate.

The report of the referee on insecticides and fungicides was read by Mr. L. A. Voorhees, the associate referee, and was a very interesting contribution to a branch of chemistry which, so far, has received comparatively little attention.

The report on dairy products was read by the referee, Mr. J. B. Weems, and added much to that branch of agricultural chemistry.

The referee on phosphoric acid, Mr. E. G. Runyan, presented a report summarizing the progress which had been made in the methods of determining phosphoric acid during the fifteen years of the existence of the Association. Especial attention was given to the development of the volumetric method, whereby the processes for estimating phosphoric acid in its usual forms are greatly shortened without any impairment of accuracy.

A similar paper relating to the determination of nitrogen was presented by Mr. F. S. Shiver, the referee on this subject.

The report of the committee on food standards was read by Mr. Frear. The report shows the method in which the work has been divided among the various subcommittees and the character of the subjects assigned to each committee. Great progress has already been made in the study of the data which must be considered in fixing food standards, and, from the amount of work which has already been accomplished, it is evident that in a very short time the Association will be in possession of a series of food standards which are based upon the most reliable data obtainable. The value of such a set of standards, especially from a legal aspect, is extremely great. One great difficulty in the enactment of pure food laws heretofore has been the incompleteness in the standards of purity. The final result of the work of the Association in this respect will be such as to warrant the acceptation and the adoption of these standards in the municipal, State and national legislation enacted in the interest of pure food.

The officers which have been elected for the ensuing year are :

President: Mr. B. W. Kilgore, North Carolina.

Vice-President: Mr. L. D. Van Slyke, New York.

Secretary: Mr. H. W. Wiley, Washington, D. C.

Additional members of the Executive Committee : Messrs. M. E. Jaffa, California ; Arthur Goss, New Mexico.

H. W. Wiley.

A CARD CENTRALBLATT OF PHYSIOLOGY.

In Science for September 1, 1899, it is stated " that the Boston Public Library will undertake the printing of a card catalogue of physiology, the cards to contain not only the ordinary bibliographical information, but also brief abstracts of the papers. The plan originated in the physiological department of the Harvard Medical School, and Professor W. T. Porter will be responsible for securing or preparing the abstracts."

This statement is inexact, and if allowed to go uncorrected would be certain to harm a useful undertaking. For this reason it seems best to give at once the details of the proposition now being considered by the Trustees of the Boston Public Library. I am the more concerned to have these details correctly understood, because the proposed method of making the literature of physiology more accessible is not limited to that science, but may be extended to all sciences, the literature of which is sufficiently compact to warrant the publication of a Centralblatt.

The need of rapid and easy access to the stores of science increases daily. The in-

vestigator is much hampered by the difficulty of collecting all the references to the work in hand, scattered as they are through large and diffuse literatures; the university lecturer finds his utterances become more fragmentary every day; and as for the advanced student, he is often dismayed by the mere account of what must be done in order to be certain of the real state of opinion concerning the subject that interests him; in fact, we are likely to be crushed under the pressure of discoveries, old and new. The present *Centralblätter* and the *Jahresberichten*, excellent as they are, afford but scant relief. In every instance the seeker must examine the indexes of a long series of volumes, and these indexes are commonly the not too laborious creation of men uninstructed in cataloguing. Indexes, moreover, are usually put together from the titles of papers, and titles rarely give a sufficient idea of the the whole contents.

But it is not my intention to write of the dark side of productiveness. The danger of swamping in the sea of literature is patent to every one. In several countries relief is being sought by the issue of card indexes. These remedial measures are alike in that they offer the titles of scientific communications, each printed on a single card, intended to be placed in an author's catalogue, *i. e.*, a set of cards arranged alphabetically by the names of authors. This function they might carry out very well, if they were printed and distributed with sufficient promptness.

Besides the author's name, and the title, date, and place of publication, some additional information usually is printed on the card so that duplicate cards may be arranged as a subject catalogue, *i. e.*, filed alphabetically according to subject. The efforts toward a subject catalogue, so far as I am acquainted with them, fall into three groups. In the first, it is proposed to give, besides the data already mentioned, a number of cross-references, in other words, a list of matters treated by the author; this, if I am correctly informed, is essentially the intention of the Royal Society. The second method proposes a few lines of text furnished by the author himself and stating his principal results; this was proposed by Professor H. P. Bowditch. The third consists of a few lines of contents, written by a cataloguer.

Any of these catalogues is undoubtedly much more useful than a bare title. From none of the three, however, can the investigator receive a satisfactory idea of the results of his predecessors. The cross-reference card is a simple index, and nothing more; the others, by reason of too great brevity, are not much better. Of the second and third method, I have some personal experience. The *American Journal of Physiology*, at Dr. Bowditch's suggestion, undertook a practical demonstration of his plan. An 'index slip' was issued with each number of the *Journal*. The slip contained the author's name, date, title and place of publication of each article in the number, and a statement of results of not more than 150 words.

Authors were invited to write their own statements. The slip was printed on thin paper, so that each statement could be cut out and pasted on a library card. In editing the slip for the *Journal*, I found that the results of many investigations could not be stated in the space allowed; in such cases investigators objected with right that the too brief statement was misleading. It appeared further that nearly all the 'copy' received from contributors had to be partially rewritten; the author who had just filled many a broad octavo page could not shrink within the limits of the library card. A number of slips I had to write myself, because the authors failed to send any statement whatever, or sent them after the slip had gone to press. I believe that the

handling of the whole current physiological literature by this method of coöperative authors would hardly be practical for the reasons already hinted; because a proper account of many a research cannot be given in the space of one library card, and an imperfect account wrongs the author and deceives the reader; and because it would be far from easy to persuade each year more than a thousand authors to send in suitable 'copy' with sufficient promptness; certainly many authors would refuse, and thus a considerable number of the cards would after all be written by the editor, who could not have first-hand knowledge of all the subjects of which he wrote.

These same objections apply also to the third method mentioned above, that of a card written by a cataloguer. The lack of space and the impossibility of really expert knowledge of all chapters in so wide a science are fatal to the best results.

These difficulties led me naturally to the idea of a Centralblatt printed on cards. In a properly organized centralblatt, the abstracts are as long as may be necessary to do justice to the author's results, and each abstract is written by an expert in the field in which the original investigation lies. The advantage of having such abstracts printed on library cards is plain. The original set, arranged alphabetically by authors' names and chronologically under each author, would give the principal results of each investigator throughout his whole career. Duplicates arranged according to cross-references printed in the upper and lower margin of each card would furnish not only a chronological list of the investigations on any particular subject, for example, on the chemical reaction of the gastric juice, but would without further search set forth the fruits of the studies mentioned. Certainly none of the methods already described approaches this one in usefulness, and its wide adoption should follow quickly on the demonstration of its practicability. This demonstration was furnished in the proposition made by me to the Trustees of the Boston Public Library.

By this proposition the Library would print on cards a Centralblatt of Physiology to be issued under the direction of a professional physiologist. The actual cost of printing is guaranteed. Thus the Library, secured from loss, would allow the manufacture on its premises of an apparatus devised to make knowledge more accessible—the end for which the Library itself was created. This permission would be valuable, because the Boston Public Library is at present better equipped for such an undertaking than any other library,* here or abroad, and because the cost of manufacture by such an institution includes neither commercial risk nor commercial profit.†

It is agreed that this card Centralblatt shall contain abstracts of original communications in physiology, including physiological chemistry, invertebrate physiology and the physiological action of drugs. Each abstract is to be written by a physiologist specially learned in the field in which the original belongs. Wherever possible the abstract is to be the work of the author himself, following the admirable suggestion of Dr. Bowditch. Abstracts are to be mailed to subscribers about three weeks after the appearance of the original in this country, and in six weeks in the case of communications published abroad. Taking an average of the abstracts in a volume of the *Centralblatt für Physiologie* and the *Jahresbreicht über die Fortschritte der Physiologie* as a guide, it is expected that 1,500 to 1,700 abstracts, requiring in all

* The most valuable part of this equipment is the skill of Mr. Francis Watts Lee, the accomplished head of the Printing Department. I am indebted to Mr. Lee for much practical information.

†The cost of issue by a commercial house not specially equipped for such work would be between $3,200 and $3,500.

about 2,000 cards of standard library size would be printed annually. The issue would be fortnightly. Each card would contain the author's name; the title; the date and place of publication; the abstract; the name of the expert writing the abstract; two cross-references, each with reference numbers according to the Dewey system; and, finally, the special data required by the mailing law. Composed in linotype brevier, a clear, easily read type, the space available for the text of the abstract would hold about 225 words. The average length of abstracts in the *Centralblatt für Physiologie* is about 200 words. Where the abstract is too long to be printed on one card, a second, or a third, would be used. A thousand cards will "bulk" nine inches.

The regular issue would consist of an original and two duplicate sets. The original set could be arranged alphabetically by the names of authors. The duplicates could be arranged by subjects, with the aid of the cross-references or the Dewey numbers. Suitably printed guide cards, and filing boxes of stout cardboard, the corners strengthened with metal, would be furnished. The price per year, *i. e.*, for about 6,000 cards, with sufficient printed guides and filing boxes, would be ten dollars, postage free, to subscribers in the United States and Canada, and twelve dollars and a half to foreign subscribers, the additional charge being the excess of foreign over domestic postage.

It is agreed that no charge would be made for editorial and business management, that the remuneration of the writers of abstracts would be merely nominal, and that any excess of receipts over expenditures would be applied toward increasing the value or diminishing the price of the publication to the subscriber. Scientists are obliged to collect all the literature of their special subjects. It is believed that the additional labor of putting these gleanings in shape for publication will be repayed in large part by the general saving of time and trouble which the new publication would undoubtedly effect. Besides, the work is a public service.

It has already been said that the Trustees of the Boston Public Library have not yet acted finally upon this proposition. In the event of their deciding that the Library shall not increase its usefulness in this particular direction, it is hoped that means will be found of printing elsewhere. The success of this undertaking in physiology would mean the issue of similar publications in other sciences and the saving of much valuable time now wasted in unprofitable rummaging.

WILLIAM TOWNSEND PORTER.

HARVARD MEDICAL SCHOOL, September 7th.

SCIENTIFIC BOOKS.

The Soluble Ferments and Fermentation. By J. REYNOLDS GREEN. Cambridge. 1899. Pp. 438. [From the Biological Series of Cambridge Natural Science Manuals.]

Enzymology, or the science of the soluble ferments, is a rapidly growing branch of physiological science. Numerous observations bearing upon it are so widely scattered through chemical, botanical, bacteriological, physiological and other journals that it is somewhat difficult to follow its progress and make a systematic summary of the subject. The books thus far published do not treat the entire subject from a physiological aspect. The work of Gamgee, published in 1893, on the chemistry of digestion, is intended especially for the *physician* and treats very ably the enzymes of the animal body, while the work of Effront, *Les enzymes et leur applications*, published in 1896, has especially the *technical* side in view, although it does not neglect the purely chemical details of recent investigations. Reynolds's book attempts more; it undertakes not only to give a detailed description of the enzymes and their actions, but also to bring before us all the physiological relations in plants and animals. It is divided into twenty-four chapters. The first treats of the nature of fermentation and

its relation to enzymes. Chapters I. to IX. treat of enzymes acting upon various carbohydrates; they comprise diastase, maltase, inulase, cytase and others. Chapter X. considers the glucoside-splitting enzymes, as emulsin, myrosin, rhamnase and others. Chapters XI., XII. and XIII. treat of proteolytic enzymes, as pepsin, trypsin, papain, bromelin, etc. Chapter XIV. describes the fat-splitting enzymes, and the following three chapters the clotting enzymes, rennet, thrombase and pectase. Then follows urease and hystozym. Chapter XIX. is devoted to the oxidizing enzymes, and Chapter XX. to the alcoholic fermentation. Chapter XXI. treats of the fermentative power of protoplasm, Chapter XXII. of the secretion of enzymes, Chapter XXIII. of the constitution of enzymes, and Chapter XXIV. of the theories of fermentation.

In the preface and first chapter the author defines his own position in regard to views on fermentation. He lays stress on the relations between "fermentation in the broad sense and the general metabolic phenomena of living organisms." Recent discoveries "have shown more and more plainly what a prominent part is played by enzymes in intracellular metabolism, till it has become clear that the distinction drawn between organized and unorganized ferments is based upon an incomplete acquaintance with the metabolic processes in both higher and lower organisms, and must now be abandoned entirely in the light of fuller knowledge." He defines consequently fermentation to be the "decomposition of complex organic material into substances of simpler composition by the agency either of protoplasm itself or of a secretion prepared by it."

While this general view hardly requires any further comment, grave doubts may be expressed as to the correctness of his general view on the characteristics of protoplasm. The author adopts the views of Pflüger and Detmer, and believes in its "continually undergoing decomposition and reconstruction." This decomposition can, however, continue for only a short time, as otherwise death would result before reconstruction could take place. To explain the principles of life as a perpetual destruction and reconstruction of protoplasm shows very erroneous conceptions. The material destroyed in the process of life consists of 99 per cent. of carbohydrates, fats and passive proteids of the food, but not of the living matter itself. The general descriptions in Green's book of enzymes and their physiological relations correspond to the relations of the plant physiologist. We find in Green's book excellent paragraphs on diastase of secretion and diastase of translocation; on the condition of secretion of diastase and the condition of the action of diastase; on cellulose, dissolving enzymes, and on vegetable trypsins. But wherever the progress of modern *chemistry* comes into play the physiological chemist will hardly be satisfied. The author ignores the principle of chemical *lability* of the proteids of living matter and in enzymes, by which property heat can be converted into mechanical energy.

The author has overlooked further certain points in recent chemical literature; otherwise he would not have mentioned (p. 168) antipeptone as a chemical substance. Recent investigations have placed beyond doubt the fact that the so-called anti-peptone is no peptone at all, but essentially a mixture of several bases, viz., arginin, lysin, histidin.

Further, Green mentions on page 174 the artificial production of an albumin from peptone by acetic anhydride, although it was shown several years ago that in this way merely an acetyl peptone, but no true albumin, is obtained.

In regard to the preparation of diastase Green describes several methods, but not without making various errors. In the first place he calls the method which makes use of basic acetate of lead for the isolation of enzyme Loew's method, while it was Wurtz who applied this method first and with great success in the isolation and purification of another enzyme papain.*

In the second place Green calls this method untrustworthy, although he did not control it by a single experiment, but merely relied upon the judgment of a young chemist, who in the first investigation he had ever published failed to obtain a powerful diastase. This was, however, due to the fact that he had not yet ac-

* *Comptes Rendus*, 90, 1379 and 91, 787

quired the necessary skill and had disregarded necessary precautions for the treatment of such a delicate substance as diastase. In the third place Green gives a detailed account of Loew's method for the preparation of diastase which is totally *erroneous*, since neither calcium salts nor caustic soda nor acetic acid mentioned in that description has ever been used in this method. Further, the diastase was not obtained from the precipitate produced by basic lead acetate, as Green states, but from the filtrate of that precipitate. Those readers who wish to compare the method in question with Green's remarkable translation are kindly referred to Pflüger's Archiv, Vol. 27, p. 206.

Another erroneous statement is found on page 113. We read there: "Invertase has been described by Atkinson and by Kellner, Mori and Nagaoka as existing in rice and in Koji, a peculiar preparation of that cereal which is much used by the Chinese in the preparation of fermented liquids." The statement that the authors noted had found invertase in *rice* is incorrect. They have found it in Koji, which consists of a growth of *Aspergillus oryzæ* on *boiled* rice, and, as these authors have proved, it is that fungus which contains the invertase, and not the rice in which this enzyme could hardly have been suspected. Further, it might be mentioned that the Japanese make relatively as much use of Koji as the Chinese.

As to the names adopted for new enzymes it may be mentioned that in Chapter IX. the name *glucase* is used for the enzyme which splits maltose into two molecules of glucose. Various chemists, however, have agreed that for the sake of uniformity the new enzymes should be named after the compound acted upon and not after the compounds resulting from this action. Consequently, the name *maltase* and not *glucase* is now in universal use with physiological chemists, which name, however, is only once mentioned in parenthesis in Green's book. In the chapter on the oxidizing enzymes we find detailed accounts of various investigations on laccase, tyrosinase, oenoxidase and animal oxidases. One very essential point, however, has been overlooked by Green, namely the distinction between oxidases and peroxidases, the former yielding *directly* a blue color with dilute guaiac tincture* in presence of air, the latter only in presence of hydrogen peroxide. The former are, therefore, to be considered as the more powerful, since the rather *indifferent* oxygen of the air can be brought into action by them, while the peroxidases want, at least in their action upon guaiaconic acid and some other compounds, —oxygen in *statu nascendi, i. e.*, oxygen in a state of motion or charged with kinetic chemical energy. That hydrogen peroxides is generally decomposed by enzymes with liberation of oxygen is known and this oxygen in *statú nascendi* is more powerful than the common oxygen of the air.

In the very ably treated chapter on the secretion of enzymes we miss the investigations of Bruno Hofer, who was the first who demonstrated the direct connection of the nucleus with the formation of the enzymes.

On page 370 an erroneous conception is ascribed to Nägeli. He never entertained the opinion that the chemical powers of the enzymes are essentially different from those of the fermenting organisms. He supposed also in the enzymes certain motions (although less energetic ones) to be the cause of their actions, and defended this view against Kunkel.† The quoting of Fischer by Green is entirely unjustifiable in this connection.

In the interest of a future edition of Green's book, which would gain by a little less bias we mention the following typographical errors:

On page 49, line 32 for Grüber, read Gruber.
" " 87, " 16 " Ganz " Gans.
" " 100, " 22 " " " "
" " 113, " 22 " Nagaoko " Nagaoka.
" " 273, " 17 " Mallevre " Mallvère.
" " 288, " 28 " Schmiedeburg read Schmiedeberg.
On page 340, line 15 for Pfluger read Pflüger.
" " 177, " 37 " Loew's method read Wurtz's method.

Notwithstanding some unjustifiable remarks and some erroneous chemical statements, the

* Guaiac tincture is a very valuable reagent in the hands of a cautious chemist who discriminates and controls. It is unreliable only in the hands of untrained persons. Above all, it has to be frequently renewed and to be kept cool in the dark.

† Sitzungsberichte der Bayrischen Akademie der Wissenschaften, 1880, p. 385.

book is a valuable contribution to the scientific literature of the subject. It can be well recommended to students of physiological science. Teachers will find in the practical arrangement of the book and in the summaries of views only to be found in widely scattered publications, a welcome guide for arranging their lectures on this subject. Investigators, however, will always prefer to consult original contributions rather than text-books or handbooks.

OSCAR LOEW.

U. S. DEPARTMENT OF AGRICULTURE.

Annals of the South African Museum. Volume I. Part 2. March, 1899.

The first of the papers (V. in the series) in this volume is 'On the Species of Opisthophthalamus in the Collection of the South African Museum, with Descriptions of Some New Forms,' by W. F. Purcell. The treatment of the genus is brought to a conclusion, three new forms are described, and the localities and local peculiarities of the specimens, numerous in the collection, are given at some length. In conclusion, the synopsis of all species known to the author, begun in a previous paper, is brought to completion. Article VI. is a 'Descriptive List of the Rodents of South Africa,' by W. L. Sclater, and is published as preliminary to a greater work on South African mammals. The genera are arranged according to the list published by Oldfield Thomas in the *Proc. Zool. Soc.* for 1896, and 62 species are mentioned, one *Malacothrix pentonyx* being new.

Article VII.—'Fifth Contribution to the South African Coleopterous Fauna,' by L. Péringuey, is devoted to the description of new Coleoptera, mostly in the collection of the museum.

Article VIII.—'On the South African Species of Peripatidæ in the Collection of the South African Museum,' by W. F. Purcell, gives full descriptions of the external systematic characters of three out of the four previously described species, with descriptions of four new species. These are *Peripatopsis leonina, P. sedgwicki, P. clavigera* and *Opisthopatus cinctipes.*

Article IX.—(by a misprint given as X.), 'A Contribution to the Knowledge of South African Mutillidæ,' by F. Péringuey, describes 26 new species. This brings the number of South African species of this family to 169, but the number of which both sexes are definitely known is only 16.

The final paper X.—'Description of a New Genus of Perciform Fishes from the Cape of Good Hope,' by G. A. Boulenger, describes and figures *Atyposoma gurneyi.*

F. A. LUCAS.

A Catalogue of Scientific and Technical Periodicals, 1665–1895. HENRY CARRINGTON BOLTON. City of Washington, Smithsonian Institution, 1897. Second edition, pp. vii + 1247.

The first edition of Dr. Bolton's catalogue, issued in 1885, has been a great aid to scientific men and to scientific research, and a second edition, with many additional titles and much revision, is very welcome. The former edition contained the titles of 4,954 periodicals, and the present edition adds about 3,600 new titles, and gives further information in regard to many of the periodicals described in the first edition. Regarding all these journals full details are given—the date of establishment, the number of volumes issued, the place of publication, the editors, etc., including a history of the vicissitudes undergone by so many scientific journals. Over 200 pages are added, giving chronological tables, a subject index, and a check-list, showing in what American libraries the more important periodicals may be found.

The first part of the alphabetical catalogue is reprinted from the plates of the first edition with certain corrections. Then in the second part are the additions that could not be inserted in the plates and the new titles. This double alphabetical index is very inconvenient. It may indeed be reasonably claimed on various grounds that stereotyping is an invention for the retardation of science. The volume appears to be remarkably free from typographical errors in spite of the difficult proof reading, but it is not free from errors in compilation. Thus if we take the three leading American journals of general science, we find it said (referring to 1895), that the *American Journal of Science* is edited by 'James D. and E. S. Dana and B. Sillimann.' The *American Naturalist* is said to

be edited by 'A. S. Packard, Jr., and Edward D. Cope.' SCIENCE is said to be edited by a committee consisting of 'S. Newcomb, I. Remsen, O. C. Marsh, C. H. Merriam, J. W. Powell.' There are also serious omissions, *e. g.*, *The Bulletin of the American Mathematical Society* and the *Bulletin of the Torrey Botanical Club.*

The value of this compilation to the scientific worker is so great that the destruction by fire of the plates and sheets would not be regretted if this should lead to a new edition.

J. MCKEEN CATTELL.

GENERAL.

THE newly formed Harper-McClure combination of New York City announces the publication of an encyclopædia which is intended to surpass even the Encyclopædia Britannica in range. It is to be hoped that the scientific articles will be entrusted to men of science as competent as the writers for the *Britannica.*

MESSRS. D. APPLETON & CO. announce for early publication the 'Comparative Physiology and Morphology of Animals,' by Professor Joseph Le Conte, and 'The International Geography' by Nansen, Markham, Bryce, Davis and others.

MESSRS. HARPER & BROTHERS have in press the 'Elements of Physics,' by Professors J. S. Ames and H. A. Rowland of the Johns Hopkins University.

BOOKS RECEIVED.

A Manual of Psychology. G. F. STOUT. London, W. B. Clive; New York, Hinds & Noble. 1899. Pp. xvi + 643.

Text-book of Vertebrate Zoology. J. S. KINGSLEY. New York, Henry Holt & Company. 1899. Pp. viii + 439.

The Teaching Botanist. WILLIAM F. GANONG. New York and London, The Macmillan Company. 1899. Pp. xi + 270.

The Elements of Blowpipe Analysis. FREDERICK HUTTON GETMAN. New York and London; The Macmillan Company. 1899. Pp. ix + 77. 60 cts.

SCIENTIFIC JOURNALS AND ARTICLES.

The Journal of Geology. May–June, 1899. Vol. VII., No. 4. The number opens with a symposium of three papers dealing with the Permian of the states west of the Mississippi. C. R. Keyes writes of the 'American Homotaxial Equivalents of the Original Permian,' pp. 321–342. A comparison is drawn between the American Permian and that of Russia as seen by the author during a recent Russian trip. C. S. Prosser follows with a paper on 'Correlation of Carboniferous Rocks of Nebraska with those of Kansas,' pp. 342–357. The author determines the relations of the Nebraska Carboniferous with the horizons which had been previously established by his careful, faunal studies in Kansas. W. C. Knight, 'The Nebraska Permian,' pp. 357–375. The writer's conclusions are that the Kansas Permian extends as a triangular area northward into Nebraska. Some tablesof fossils are given W. H. Hobbs, 'The Diamond Fields oj the Great Lakes,' pp. 375–389. All the known finds of diamonds in the drift of the region of the Great Lakes are recorded and plotted with the intention of locating their probable source and home, and of arousing interest in the subject. W. H. Turner, 'Replacement Ore Deposits in the Sierra Nevada,' pp. 389–401. A number of gold-bearing deposits in California are described, which are in contrast with the usual quartz veins and which give ground for an explanation by replacement. Editorials, reviews, and a valuable summary of current North American Pre-Cambrian literature by C. K. Leith conclude the number.

THE *Educational Review* for September opens with an address given by Dr. W. T. Harris before the recent meeting of the National Educational Association, outlining an education policy for our new possessions, and an article on the educational progress of the year by Professor Nicholas Murray Butler, presented to the National Council of Education. The number also contains articles on the educational system in Chicago, women in the public schools, English in Regents' schools and the teaching of German in Germany.

DISCUSSION AND CORRESPONDENCE.

THE PROPER NAME OF THE POLAR BEAR.

TO THE EDITOR OF SCIENCE: Under this heading in SCIENCE for August 15, 1899, Mr. James A. G. Rehn states that the name of the Polar Bear should be *Thalarctos marinus* (Pal-

las), giving as the reason that Linnæus did not give the name *Ursus maritimus*, as usually attributed to him, and that "the next date when any mention of the Polar Bear was made was 1776, when Müller and Pallas each gave it a name." Allow me to suggest that Mr. Rehn's statement is not correct. It is true that Linnæus did not give a binomial term to the Polar Bear, but it is not true that it did not receive one until 1776.

In 1773 Sir C. J. Phipps undertook a voyage to Spitzbergen, and in the account which he published in 1774 * the Polar Bear, with full references to Linnæus and Pennant and measurements from specimen, is formally named *Ursus maritimus*. I have not now at hand the original edition, and consequently cannot quote the page, but I possess the French translation of 1775, † in which the name occurs on p. 188. I may add that Mr. Rehn's is not the first attempt at resurrecting Pallas's name. It was done as early as 1844 by Keyserling and Blasius, who have been followed, among others, by Nilsson and Pleske. But the name of the Polar Bear should be *Thalarctos maritimus* (Phipps).

LEONHARD STEJNEGER.

U. S. NATIONAL MUSEUM,
WASHINGTON, D. C., August 26, 1899.

* Phipps (C. J.). A voyage towards the North Pole undertaken by H. M's command, 1773. London, 1774. 4to.

† Voyage au Pole Boréal, fait en 1773 * * * etc. Paris, 1775. 4to.

THE MENTAL EFFECTS OF THE WEATHER.

IN reading the interesting article by Dr. Dexter in SCIENCE of August 11th under the above heading two or three suggestions as to the causes in action occurred to me and are perhaps worth mentioning. Dr. Dexter's curves show that the greatest number of assaults occur between the temperatures of 50° and 90° F., with a maximum between 70° and 80°. There is a greater number on clear and partly cloudy days than on days with rain. In other words, the conditions which we know attract people out of doors and bring them in contact are the conditions which, it appears, produce the greatest number of assaults and thus, perhaps, are indirectly the cause of them. Even the slightly greater number of assaults on partly cloudy days than on clear days may be interpreted from this standpoint. The partly cloudy days average warmer than the clear days, and people are more attracted out of doors on mild days than on cold days.

The number of errors in banks is found to be greater on cloudy and on rainy days than on clear days. This may be due to the greater amount of light on clear days by means of which the figures are more clearly seen.

The number of people insured appears to be less on rainy days. Again, this may be explained by a tendency of rainy days to keep people in doors. A man having decided to have his life insured on a certain day which proves to be rainy postpones it to a pleasant day, when traveling is more agreeable.

The increased energy which people feel on sunny days also no doubt contributes to the effects described above, as suggested by Dr. Dexter.

The number of deaths he found to increase with the temperature when the temperature was above 80°, and in this case there is probably a direct relation of cause and effect.

H. HELM CLAYTON.

BLUE HILL METEOROLOGICAL OBSERVATORY,
September 5, 1899.

NOTES ON PHYSICS.

PHOTOGRAPHY OF SOUND WAVES.

PROFESSOR R. W. WOOD publishes in *Phil. Mag.*, August, '99, some very interesting photographs of sound waves, taken by a modification of the method of Toepler. A telescope objective pointed at a star appears as a uniformly illuminated field to an eye situated at or a trifle behind its focus, inasmuch as light enters the eye from every part of the objective. A condensed mass of air between the lens and the focus turns the light which passes through it to one side of the focus; this deflected light may be cut off by means of a screen, the edge of which just grazes the focus, and the portion of the field of view which is covered by the condensed mass of air appears dark. Instead of the eye an ordinary photographic camera may be focussed upon the telescope objective, and by using the light from an electric spark, instead of

the light from a star, an instantaneous photograph may be taken of a sound wave moving across the field of view.

Professor Wood has in this way obtained photographs showing the wave-front forms which occur in various cases of reflection, refraction and diffraction. An auxiliary electric spark is employed as a source of the sound waves to be photographed.

THE HYDROLYSIS OF STANNIC CHLORIDE.

SOLUTIONS of stannic chloride show abnormally low freezing points. Mr. Wm. Foster* has shown that this abnormal behavior is to be ascribed to the hydrolysis of the stannic chloride, namely, the formation of HCl and stannic oxide, so that in dilute solution there is slowly formed four dissociated molecules of HCl instead of one dissociated molecule of $SnCl_4$. The freezing-point constant calculated upon this assumption is 14.06 and the value observed by Loomis is 14.25.

THE SPECIFIC HEAT OF SOLUTIONS.

PROFESSOR MAGIE † has shown theoretically that the heat capacity of a solution of a non-electrolyte, osmotic pressure being proportional to the absolute temperature, is the sum of the heat capacities of the solvent and of the solute, and he has derived an expression for the change in heat capacity of any solution due to change in concentration when the relation between osmotic pressure and temperature is given.

In case of non-electrolytes the above-mentioned relation is verified by experiments of Marignac and by more acurate measurements carried out by the author. Professor Magie points out that sufficient data are not at present at hand to verify the more general relation mentioned above.

MAGNETISM AND STRETCH-MODULUS OF STEEL.

STEVENS and Dorsey ‡ have shown that the stretch modulus (Young's modulus) of iron and steel is very slightly increased by magnetization in the direction of the stretch.

W. S. F.

* *Physical Review*, IX., p. 41.

† *Physical Review*, IX., p. 65.

‡ *Physical Review*.

*THE BACILLUS ICTEROIDES AS THE CAUSE OF YELLOW FEVER.**

Sanarelli reproaches me with not being willing to yield to the evidence in favor of the specific etiological rôle of his bacillus. I am not influenced in my scientific conservatism by any feelings of jealousy, and shall be ready to do full honor to the discoverer when the discovery is definitely established. At present I cannot admit this for the following reasons:

First. Sanarelli's bacillus grows readily in the culture media employed by me in my researches, but in nineteen typical cases of yellow fever in which I introduced into such media blood from the heart of yellow-fever cadavers, this bacillus was not present, the cultures remaining sterile in fifteen. In three of the four cases in which a growth occurred, I identified the bacillus found as bacillus coli communis (my bacillus *a*). I strongly suspect that some of those bacteriologists who claim to have found Sanarelli's bacillus have mistaken for it one of the varieties of the colon bacillus.

Second. In my experiments material from the interior of the liver and kidney, containing blood and crushed tissue elements from fresh cadavers, was added to culture media in which Sanarelli's bacillus readily grows, but I obtained a negative result (cultures remained sterile) in 30 out of 43 cases.

Third. Sanarelli's bacillus is fatal to guinea-pigs and rabbits when injected subcutaneously in very minute doses. In my experiments blood from the heart and crushed liver tissues from the fresh cadaver failed to kill eight out of ten guinea-pigs and seven out of eight rabbits experimented upon. I admit that the value of these experiments is impaired by the fact my laboratory facilities did not permit me to keep these animals under observation as long as was desirable.

Fourth. The experiments made by Drs. Reed and Carroll at the Army Medical Museum, show that Sanarelli's serum in high dilutions, (1–100,000) causes arrest of motion and typical agglomeration (Widal reaction) of theb acillus of hog cholera; also, that serum from an animal immunized against hog cholera, in high dilu-

*From a reply to Professor Sanarelli, by Dr. George M. Sternberg, published in the *Medical News*.

tions, causes arrest of motion and typical agglomeration of Sanarelli's bacillus; also, that cultures of Sanarelli's bacillus fed to pigs cause the death of these animals, and that the typical lesions of hog cholera are found in their intestine.

Fifth. The blood-serum of yellow fever patients or of convalescents from this disease does not give a marked Widal reaction with Sanarelli's bacillus, although the blood of an animal immunized by the injection of cultures of this bacillus does give the specific reaction in high dilution.

Sixth. So far as I am informed the results obtained in the use of Sanarelli's antitoxic serum do not give support to his claim to have discovered the specific germ of yellow fever.

In a letter dated January 20, 1899, my friend Dr. J. B. de Lacerda, of Rio de Janeiro, says:

"The serum of M. Sanarelli has failed here in Brazil. The results of the experiments which he made at San Paulo have not recommended the employment of this serum. It is neither preventive nor curative."

In a paper recently published in the *New Orleans Medical and Surgical Journal*, Dr. P. E. Archinard reports a negative result from the use of Sanarelli's serum in ten cases. He says:

"From the above cases, which limit our experience with the anti-amarylic serum of Sanarelli as a curative agent in the human being attacked with yellow fever, we are forced to conclude that this agent, in our hands, has shown no curative powers whatsoever, none of the important and dangerous symptoms of the disease having been in any way mitigated or prevented by its administration."

Drs. Reed and Caroll are now preparing a report of their extended researches, which have been going on at the Army Medical Museum during the past two years. This report will be published in due time and will give full details as to the experimental evidence upon which they base their conclusion that Sanarelli's bacillus is a variety of the bacillus of hog cholera.

Finally, I would say it appears to me at the present time that, like the colon bacillus and bacillus *x*, the bacillus of Sanarelli is a pathogenic saprophyte which is present occasionally and accidentally in the blood and tissues of yellow-fever patients, and that its etiological relation to this disease has not been established. If, however, the results reported by Drs. Reed and Carroll can be shown to be based upon erroneous observations, I shall be ready to revise my opinion. Truth is mighty and no doubt in the end will prevail.

INTERNATIONAL CONGRESS ON TUBERCULOSIS.

THE report of Sir Herbert Maxwell, M. P., F.R.S., and Dr. Pye-Smith, F.R.S., the delegates of the British government at the International Congress on Tuberculosis held at Berlin from May 24th to 27th last, has been issued as a Parliamentary paper. The report states, as abstracted in the London *Times*, that the Congress, which was opened by the Herzog von Ratibor, in the presence of the German Empress, consisted of 180 delegates, appointed by and representing different states and universities and other public bodies. A number of papers were read, chiefly by German delegates, but nothing in the nature of a general discussion took place. The proceedings when printed will form a valuable *corpus* of scientific opinion on the subject.

Dr. Pye-Smith adds a memorandum on the medical aspect of the results of the Congress. After giving in some detail the most important conclusions which were recognized—that consumption and other tubercular diseases are caused by the presence and multiplication of the specific bacillus discovered by Professor Koch; that tuberculosis, as a condition directly transmitted by inheritance, is extremely rare; and that phthisis, or pulmonary tuberculosis, in particular, is not catching—Dr. Pye-Smith goes on to describe the following practical points in the prevention of tuberculosis as a widespread and destructive disease which were inculcated by various speakers at the Congress:

A. The primary importance of free ventilation and wholesome and abundant food. Improvement in the dwellings and the food of the poorer classes in this country, and their increasing cleanliness and sobriety, have not only diminished sickness generally, but directly reduce the number of deaths from consumption

until the mortality from this cause is less in London than in any other large city. (It is, however, important to notice that the death-rate of young children from disease of the bowels has little, if at all, diminished. See Sir Richard Thorne's Harben Lectures.)

B. The prevention of infection of the lungs by the bacillus of tubercle depends chiefly on the rational treatment of the sputa of consumptive patients, or rather, for practical purposes, of the sputa of all those affected with cough and expectoration. The phlegm should never be deposited on the ground or on a handkerchief, where it can dry up; it should be kept moist until it can be destroyed by heat, and the vessel used to receive it should contain phenol or some other antiseptic solution.

C. The prevention of infection by tuberculous milk may be accomplished either by boiling all milk given as food to children or by inspection of dairies, so as to prevent tuberculous milch-cows being used.

D. The prevention of infection by meat can be secured by careful and thorough inspection of carcasses, or by diagnostic testing of cattle with tuberculin. This, the only undoubtedly useful application of the so called tuberculin, has the drawback that after the effect of the inoculation has passed off a tuberculous animal becomes immune to it for a time, and so may be passed as healthy. (It is said that cattle suspected of tubercle are thus rendered immune to the tubercular test before being sent over the French frontier.)

Though the question of the treatment of phthisis was only a supplementary part of the work of the Congress, Dr. Pye-Smith gives the following facts, which are, he says, "important for the people as well as their governors to be aware of":

a. That tuberculous disease of the bones and joints of the glands and skin and abdomen, though dangerous, is not incurable, and, by the modern methods of operative medicine, is in most cases successfully cured.

b. That tuberculosis of the lungs (phthisis, or consumption) is frequently cured, and probably more often now than formerly. (Curschmann, of Leipzig, fourth day of Congress.)

c. That there is no specific drug which has direct influence upon consumption, but that many, both old and new, have valuable effects upon its complications. (On the Action of the New Tuberculin, see Briger's paper, on the second day of Congress, and Dr. C. T. Williams in the R. Med. Ch. Trans. for the present year.)

d. That abundant food, particularly of a fatty nature, and a life in the open air, are no less valuable in the treatment than in the prevention of phthisis, and that the hospitals and asylums for providing these essentials, which are now numerous in Germany, and far from rare in England, Austria and Hungary, France and the United States, are of essential value. That the 'open-air treatment' has been long known and practiced in the United Kingdom was handsomely acknowledged by Professor Von Leyden (first day of Congress). Compare papers by Kaurin (Norway), Westhoven (Ludwigshaven), J. R. Walters (London), Desider Kuthy (Budapest), Schmidt (Switzerland), Dómene (Spain), fourth day.

e. That the influence of climate, altitude, temperature, and dryness of the air and soil, of travelling and of sea voyages has been very differently estimated at different periods, and that, while each is in various degrees important, popular opinion probably exaggerates their power. (Herman Weber, of London, fourth day of Congress.)

f. That the prospect of improved success in the treatment of tuberculosis in general, and of consumption in particular, by the advance of pathology and the progress of surgery and medicine, is a hopeful one, almost as hopeful as that of limiting the spread of the disease by preventive measures.

SCIENTIFIC NOTES AND NEWS.

PROFESSOR SIMON NEWCOMB has been elected president of the Astronomical and Astrophysical Society of America, organized last week at the Yerkes Observatory, in succession to the Conferences of Astronomers and Astrophysicists which met last year at the Harvard College Observatory and the preceding year at Yerkes Observatory.

THE delegates of the National Geographic Society to the Seventh International Geograph-

ical Congress, which will be held at Berlin from Thursday, September 28th, to Wednesday, October 4th, under the auspices of 'die Gesellschaft für Erdkunde zu Berlin,' are as follows: Dr. Alexander Graham Bell, President of the Society; Gen. A. W. Greely, U.S.A., also designated by President McKinley to represent the United States Government; Professor Willis L. Moore, Chief of the Weather Bureau; Hon. Andrew D. White, U. S. Ambassador to Germany; Miss Eliza Ruhamah Scidmore, Foreign Secretary of the Society; Mr. Marcus Baker, of the U. S. Geological Survey; Dr. L. A. Bauer, of the U. S. Coast and Geodetic Survey; and Professor Wm. M. Davis, of Harvard University.

THE German government has sent Professor von Volkens, of the University of Berlin, to the Caroline Islands to investigate the soil and the flora.

ALBERT B. PRESCOTT, professor of chemistry in the University of Michigan, was elected president of the American Pharmaceutical Association at its meeting last week at Put-in Bay, Ohio.

WE regret to record the following deaths: Dr. Karl Bernhard Brühl, formerly professor of zootomy in the University of Vienna, on August 14th, aged 80 years; Professor Erhardt, formerly director of the Museum of Natural History at Coburg, aged 80 years, and Professor Oluf Rygh, who held the chair of archæology at Christiania.

MR. O. G. JONES, instructor of Physics in the City of London School, has been killed by an Alpine accident on the Dent Blanche near Zermatt.

THE British Association has this week begun its meeting at Dover. According to preliminary announcements, Professor Michael Foster, the President, will compare the condition of science in 1899 and 1799, and will dwell upon the intellectual influence of science and its value as mental training. He will also consider the benefits of international efforts. The addresses by the presidents before the Sections will be as follows: Mathematics and Physics, Professor J. H. Poynting, on the nature of law, explanation and hypothesis as used in physical science; Chemistry, Mr. Horace T. Brown, on the assimilation of carbon by the higher plants; Geology, Sir Archibald Geikie, on geological time; Zoology, Professor Adam Sedgwick, on variation in phenomena connected with reproduction and sex; Geography, Sir John Murray, on the floor of the ocean; Political Economy and Statistics, Mr. Henry Higgs, on the consumption of wealth; Mechanical Science, Sir H. W. White, on steam navigation at high speeds; Anthropology, Mr. C. S. Read, subject not announced; Physiology, Mr. J. M. Langley, on the motor nerves; Botany, Sir George King, on systematic botany in India. Professor Ch. Richet will give a lecture on nervous vibration, and Professor Fleming one on the centenary of the electric current. The usual lecture to working men will this year be omitted.

THE systematic effort begun in June by the National Geographic Society toward the enlargement of its work by increasing its membership throughout the country is proving most successful. Within the last three months over 375 non-resident members have been enrolled, representing every state in the Union and different sections of Canada. The membership of the Society is now about 2,000.

THE 18th Annual Congress of the Sanitary Institute of Great Britain opened at Southampton on August 29th with about 1,700 members in attendance. The president, Sir W. H. Preece, made the annual address, in which he discussed pure air, pure water, pure food, pure soil and pure dwellings.

THE burning of the buildings of the Volta Centenary Exposition, at Como, will not, as we have already stated, prevent the holding of the electrical congress, which opens on the 18th inst. Professor Righi will open the congress with a commemorative address on Volta. As part of the proceedings there will be a discussion on electrical terminology.

THE American Museum of Natural History, New York City, has now twenty-three representatives in the field engaged as follows: The Jesup expedition to the North Pacific making archæological and ethnological researches in British Columbia and Northeastern Siberia; the Jesup zoological expedition to the United

States of Columbia; the Constable expedition to the Northwest for large mammals; an expedition to New Mexico to study the cliff dwellings and the Pueblos; an expedition for the study of North American Indians in California and Arizona; a paleontological expedition to Wyoming; an expedition to Peru and Bolivia under Dr. Bandelier, and lastly local archæological work.

IT is reported that the explorer, Professor Wilhelm Joest, who died some time ago during an expedition among the South Sea islands, has left $75,000 to the Ethnological Museum in Berlin. The interest of that sum is to be used for getting new collections and assisting scientific expeditions.

NEWS has been received from the steamship, *Windward*, which has arrived at Newfoundland from North Greenland, and from the steamship *Diana*, which arrived at Cape Breton on the 12th. The two steamships met at Etah on August 12th, and under Lieutenant Peary's direction made arrangements for the winter and for the explorations in the spring. The *Windward* was ice-bound in Allman Bay about fifty miles north of Cape Sabine, from August 18, 1898, to August 2, 1899. During this period Lieutenant Peary made sledging journeys aggregating more than 1,500 miles, including a visit to Fort Conger, headquarters of the Greely expedition. The *Fram* was also at Etah at the same time as the other two steamships.

THERE will be a U. S. Civil Service examination on October 4th to fill the position of assistant physician in the Government Hospital for the insane. One man is wanted at a salary of $900, and one woman at a salary of $600.

THE new white star steamship *Oceanic* sailed from Liverpool for New York on the 6th inst., with 1,400 passengers. The steamship is the largest afloat, its tonnage being 17,000 and its length 704 feet.

EXPERIMENTS in wireless telegraphy are being made between the Blue Hill Observatory and Cambridge, the wires at the Blue Hill Observatory being attached to kites.

A COMPETITION has been held in Liverpool for motor vehicles invented for heavy traffic. Distances from [illegible] to 40 miles were traversed on two successive days, and six motors, all using steam, took part. The Steam Carriage and Wagon Company was given gold medals, both for vehicles having a minimum load of two tons, and for those having a minimum load of six and one-half tons.

IT is reported that an International Sanitary Commission will meet at Brussels during the present month to discuss measures for preventing the spreading of the plague in Europe.

WE learn from the *Botanical Gazette* that Mr. J. N. Rose, who was accompanied by Dr. Walter Hough, has just returned from a three months' trip through Mexico, bringing about nine hundred species of dried plants, many living plants and plant photographs. Besides rediscovering *Echmocactus Parryi*, he collected several other species lost or hitherto unknown to American herbaria. About 200 species were collected at type localities. Mr. Rose made a thorough study of the species of agave, especially those used in the manufacture of pulque and mescal.

THE special committee on Weights and Measures at the recent meeting of the American Pharmaceutical Association, submitted the following report:

No action has been taken during the past year, by the legislative branch of our Government in regard to the adoption of the Metric System of Weights and Measures.

The bill formerly before Congress, making the Metric System the legal system of weights and measures in the United States, is still in the hands of the Committee on Coinage, Weights and Measures.

Notwithstanding the inactivity of Congress on this question, we are pleased to report a healthy growth in the sentiment favoring the use of the Metric System both in medicine and for general usage.

In many of the reports made to the President by United States Consuls, the importance of the adoption of the Metric System by the United States for commercial purposes, is dwelt upon and strongly recommended. In a number of recent medical journals, editorials have been published advocating its adoption by the medical colleges.

While no definite statement can be made as to the probable action by the next Congress of the United States, or by the new Committee on Coinage, Weights and Measures yet to be appointed, it is hoped

by your committee that some definite advance can be made toward the adoption of the Metric System during the sessions of the Fifty-sixth Congress.

UNIVERSITY AND EDUCATIONAL NEWS.

THE plans for building the University of California, submitted by M. Bernard, of Paris, have received the first prize in the competition arranged by Mrs. Phoebe Hearst. The cost of the buildings is estimated at over $15,000,000. Contrary to the statements in the daily papers we understand that Mrs. Hearst has not as yet undertaken to defray the cost of any of the new buildings.

IN addition to $300,000 subscribed from various sources for an endowment of Brown University, made on condition that $2,000,000 be collected, Mr. John D. Rockefeller has offered to give $250,000 on condition that $1,000,000 be raised before commencement of next year.

ACCORDING to a dispatch in the daily papers Mr. S. F. Loubat, of New York, now residing at Paris, has given 300,000 Marks to the University of Berlin to endow a professorship 'for Americans,' which probably means for 'Americana,' a subject in which Mr. Loubat is much interested.

A CORRESPONDENT of the London *Times* sends the epitaph of the founder of Yale University from his tombstone in Wrexham Churchyard, North Wales, which is as follows:

Eliugh Yale, Esq., was buried the twenty-second of July in the year of our Lord MDCCXXI.

'Born in America, in Europe bred,
'In Africa travelled, in Asia wed,
'Where long he lived and thrived, in London dead.
'Much good, some ill, he did, so hope all's even,
'And that his soul through mercy's gone to Heaven.
'You that survive, and read this tale, take care
'For this most certain exit to prepare,
'Where blest in peace the action of the just
'Smell sweet, and blossom in the silent dust.'

IT was remarked in a recent number of SCIENCE that "the most disappointing aspect of university education seems to be the complete lack of medical students who take higher degrees." The observation appears to be enforced by the fact that three of the students who this year received the Ph.D. in psychology at Columbia University have been appointed to assistantships in the physiological laboratories of medical schools--Drs. G. V. N. Dearborn and S. I. Franz in the Harvard Medical School, and Dr. R. S. Woodworth in the University and Bellevue Hospital Medical College.

DAVID R. MAJOR, Ph.D. (Cornell), who was last year fellow in education at Teachers College, Columbia University, has been appointed acting professor of pedagogy in the University of Nebraska.

L. C. GLEN, Ph.D. (Johns Hopkins), has been appointed professor of geology at South Carolina College. F. A. Saunders, Ph. D (Johns Hopkins), has been appointed instructor in physics in Haverford College.

MR. EDGAR R. CUMMINGS, a recent graduate of Union College, has been appointed an instructor in geology in Indiana University. Mr. Cummings has published papers on the geology of the Mohawk Valley, N. Y., and is planning original work in the stratigraphical geology and paleontology of Indiana.

PROFESSOR W. S. MILLER, of the University of Wisconsin, has declined a call to the chair of anatomy and histology in the University of the State of Missouri.

DR. ALEXANDER MCADIE, of the San Francisco Weather Bureau, has been appointed honorary lecturer in meteorology in the University of California.

AT the University of North Carolina, J. E. Latta, B.Ph. (University of North Carolina), has been appointed instructor in physics, and Thomas Clarke, B.S. (University of Carolina, '96), Ph.D. (Brown University, '98), instructor in chemistry.

DR. ADOLF MIETHE, of Braunschweig, has been appointed professor of photo-chemistry in the Technical Institute at Berlin.

THE following have qualified as docents in German universities: Dr. Kauffman in physics at Göttingen; Dr. Henneberg in anatomy at Geessen, and Dr. Göttler in mathematics at Munich.

DR. OTTO KRIGAR-MENZEL, docent in physics in the University of Berlin, has been appointed to an associate professorship.